

AMERICAN SOCIOLOGICAL REVIEW

Culture Case Study and Greek History *Howard Becker*

The International Study of Urbanization *Jack P. Gibbs and Kingsley Davis*

Inter-Generational Mobility *Gerhard E. Lenski*

Political Activity in a Voluntary Association *Herbert Macoby*

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Contents

Culture Case Study and Greek History: Comparison Viewed Sociologically	HOWARD BECKER	489
Conventional Versus Metropolitan Data in the International Study of Urbanization	JACK P. GIBBS AND KINGSLEY DAVIS	504
Trends in Inter-Generational Occupational Mobility in the United States	GERHARD E. LENSKI	514
The Differential Political Activity of Participants in a Voluntary Association	HERBERT MACCOBY	524
Public Health as a Career in Medicine: Secondary Choice Within a Profession....	KURT W. BACK, BERNARD S. PHILLIPS, ROBERT E. COKER, JR., AND THOMAS G. DONNELLY	533
Ethnic Groups and the Practice of Medicine	STANLEY LIEBERSON	542
Fertility Through Twenty Years of Marriage: A Study in Prediction Possibilities	CHARLES F. WESTOFF, PHILIP C. SAGE, AND E. LOWELL KELLY	549
Postmarital Consequences of Premarital Sex Adjustments.....	EUGENE A. KANIN AND DAVID H. HOWARD	556
Marital Adjustment: A Factor Analysis Study.....	HARVEY J. LOCKE AND ROBERT C. WILLIAMSON	562

RESEARCH REPORTS AND NOTES

Components of Population Change in Large Metropolitan Suburbs	LEO F. SCHNORE	570
The Relation of Indices of Fetal and Infant Loss to Residential Segregation: A Follow-Up Report.....	ALFRED YANKAUER AND NORMAN C. ALLAWAY	573

COMMUNICATIONS

Suicide and Occupation	BERTHOLD BRENNER	579
Terminological Problems	HOWARD BECKER	580

THE PROFESSION: REPORTS AND OPINION

The Concepts of Culture and of Social System.....	A. L. KROEBER AND TALCOTT PARSONS	582
Occupational Roles of Sociologists.....	RAYMOND V. BOWERS	583
Official Reports and Proceedings		
Notice Concerning the 1959 Annual Meeting.....		584
News and Announcements		
Else Frenkel-Brunswik, 1908-1958.....	LEO LOWENTHAL	585

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BOOK REVIEWS

Meisel: <i>The Myth of the Ruling Class</i> . Renzo Sereno	591
Dahrendorf: <i>Soziale Klassen und Klassenkonflikt in der Industriellen Gesellschaft</i> . Kurt B. Mayer	592
Ortega Y Gasset: <i>Man and People</i> . Francisco Ayala	593
Angell: <i>Free Society and Moral Crisis</i> . William Kornhauser	593
Pepper: <i>The Sources of Value</i> . Charles Morris	594
Hays: <i>From Ape to Angel</i> . David F. Aberle	595
Schramm: <i>Responsibility in Mass Communication</i> . Ernest van den Haag	596
Key: <i>Politics, Parties, and Pressure Groups</i> . Donald R. Mathews	596
Ehrmann: <i>Organized Business in France</i> . T. B. Bottomore	597
Duverger: <i>The French Political System</i> . Stanley Rothman	598
Abegglen: <i>The Japanese Factory</i> . Marion J. Levy, Jr.	598
Hagen: <i>Handbook for Industry Studies</i> . Marion J. Levy, Jr.	598
Walker: <i>Toward the Automatic Factory</i> . Ely Chinoy	599
Pollock: <i>Automation</i> . Ely Chinoy	599
Myrdal: <i>Rich Lands and Poor</i> . Vincent Heath Whitney	600
Sanders: <i>Collectivization of Agriculture in Eastern Europe</i> . Joseph C. Rousek	601
Collins: <i>Coloured Minorities in Britain</i> . Anthony H. Richmond..	602
Townsend: <i>The Family Life of Old People</i> . Mirra Komarovsky....	603
Donahue et al: <i>Free Time</i> . Gordon F. Streib	603
Drake: <i>The Aged in American Society</i> . Gordon F. Streib	603
Bernert: <i>America's Children</i> . Harry V. Ball	605
Webster: <i>Urban Planning and Municipal Public Policy</i> . Earl E. Munzt	605
Loring et al: <i>Community Organization for Citizen Participation in Urban Renewal</i> . Earl E. Munzt	605
Landis: <i>Introductory Sociology</i> . Jiri Nehnevajsa	606
Mercer: <i>An Introduction to the Study of Society</i> . Jiri Nehnevajsa..	606

BOOK NOTES

Strausz-Hupé and Hazard: <i>The Idea of Colonialism</i> . M. A. M.	608
Tillion: <i>Algeria</i> . M. S. O.	608
Vidart: <i>La Vida Rural Uruguaya</i> . J. V. D. Saunders	608
Dube: <i>India's Changing Villages</i> . A. H.	608
Bateson: <i>Naven</i> . A. H.	609
Firth: <i>We, The Tikopia</i> . A. H.	609
Warner: <i>A Black Civilization</i> . A. H.	609
Ogburn and Nimkoff: <i>Sociology</i> . Bulkeley Smith, Jr.	609
Taft: <i>Otto Rank</i> . Dilman J. Doland	609

PUBLICATIONS RECEIVED

(Articles in the AMERICAN SOCIOLOGICAL REVIEW are indexed in the International Index to Periodicals and in the Weekly Bulletin of Public Affairs Information Service.)

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CULTURE CASE STUDY AND GREEK HISTORY: COMPARISON VIEWED SOCIOLOGICALLY

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1. BURDENS GRIEVOUS TO BE BORNE

CULTURE¹ case study is a procedure of social-scientific research. As such, it avoids the handling of cases as though they were self-contained wholes grasped by intuition. Actually, such intuition rests on implicit rather than explicit definitions of the object to be studied; what is taken to be a whole is so taken because of a host of unavowed assumptions. Further, such intuition involves purposes other than the strictly scientific: therapeutic, esthetic, appreciative, defensive, or reformist.² As the purposes

¹ Culture is here given the *general* meaning current in many circles ever since Pufendorf. See Joseph Niedermann, *Kultur: Werden und Wandlungen des Begriffes und seiner Ersatzbegriffe von Cicero bis Herder*, Florence: Biblioteca dell' "Archivum Romanicum," Serie I, Vol. 28, "Bibliopolis," Libraria Antiquaria Editrice, XIX, 1941; and John Gillin, editor, *For a Science of Social Man*, New York: Macmillan, 1954—hereinafter FASOSM—pp. 115-128.

It should also be noted that culture is used in one of the senses assignable to "normative." See Howard Becker and Alvin Boskoff, editors, *Modern Sociological Theory in Continuity and Change*, New York: Dryden, 1957—hereinafter MSTICAC—Chapter 6, esp. pp. 139-143.

² Here the reference is to such writings as those of M. J. Herskovits, Ruth Benedict, Margaret Mead, Robert Redfield, and similar exponents of *Gestalt* psychology applied to logical problems. See the writer's "Field Work among Scottish Shepherds and German Peasants: 'Wholes' and Their Handicaps," *Social Forces*, 35, 1 (October, 1956), pp. 10-15; *Systematic Sociology on the Basis of the Beziehungslehre and Gebildelehre of Leopold von Wiese*, New York: Wiley, 1932—hereinafter WBSS—pp. 78-93, 423, 656; and *Through Values to Social*

vary, the intuited whole varies. Nevertheless, culture case study does deal with cases, and therefore the composite term is expressly retained. In many circles, however, opinions about the ways in which cases have often been handled are distinctly *unfavorable*. Consequently, reference to culture case study as a valuable and frequently indispensable procedure when comparison of and generalization about social phenomena are to be undertaken³ must be justified in detail.

Further, to offer suggestions about the use of historical materials for comparative purposes is a formidable task for an American sociologist—for many reasons. Here it merely may be noted that, with a few distinguished exceptions,⁴ we have long fought

Interpretation, Durham, N. C.: Duke University Press, 1950—hereinafter TVTSI—pp. 140, 145, 157-158, 179-185, 203-204, 217, 250.

³ Sociology has long been committed to procedures involving explicit comparison. See John Doby, editor, *Introduction to Social Research*, Harrisburg, Pa.: Stackpole, 1954, chapters by Francis and by McKinney; and TVTSI, Chapters 2, 4, and 5.

Other disciplines, however, have comparative interests, although comparison is usually implicit rather than explicit, and rarely on a definite procedural basis. History, for example, has never really forsaken comparison, in spite of the dictum (in the writer's view, correct), "History as history never repeats itself." See Maurice Mandelbaum, *The Problem of Historical Knowledge*, New York: Liveright, 1938, and accompanying bibliography; and TVTSI, Chapter 3, "Problems of Social Change as Viewed by Historian and Sociologist."

⁴ Barber, Eliot, Hertzler, MacIver, Meadows, Merton, Nisbet, Teggart, and Sorokin come readily to mind, but there are a few others.

shy of history. Such adverse attitudes are often intensified where ancient history is concerned. Frequently stemming from high-school days, they have taken shape in disregard of the mass of primary source materials that illuminate some aspects of ancient societies more brightly than do the scanty documents recording certain phases of our own quite recent past or even some nooks and corners of what we call the present.⁵

Consequently, those among us who claim that attention to the data yielded by history may often be sociologically rewarding should take account of sociological skeptics. Space forbids, however, more than a few illustrative references to studies in "historical sociology,"⁶ published, unpublished, and in process. Further detail is indicated by the footnotes, many of which refer to historical records as such, but others to treatments of these records by historians and, in a few instances, by "historical sociologists." The focus, for what seem to be good reasons, will be on ancient Greece.⁷

Still another burden must be shouldered—this time, where historians are concerned. Being thoroughly informed about their fields, they cannot help but be impatient with suggestions about methods of comparison and generalization coming from those lacking

⁵ What is "past" and what is "present," to say the least, are difficult to specify. See TVTSI, pp. 186-187. Because of such difficulty, it seems quite unprofitable to define history as the study of man's "past," and certain other social sciences as concerned only with the "present." The same is true of "historical" and "contemporary."

⁶ Referring to the use of "historical sociology" in the text here and elsewhere, the writer calls attention to the fact that he does not like the term, for many reasons. "Sociological study of history" seems preferable, but unfortunately is cumbersome.

⁷ From time to time the writer has given evidence of this interest: "Ionia and Athens: Studies in Secularization" (unpublished Ph.D. dissertation, University of Chicago, 1930); *Social Thought from Lore to Science*, with H. E. Barnes, Boston: Heath, 1938, 1st ed.; Washington, D. C.: Harren, 1952, 2nd ed.; New York: Dover Publications, 2nd ed. reissue, 1956, Chapter 4, "Mental Mobility in the Greco-Roman World;" "In Defense of Morgan's 'Grecian Gens': Ancient Kinship and Stratification," *Southwestern Journal of Anthropology*, 6, 3 (Autumn, 1950), pp. 309-339; and "Church and State in the Cosmos of Crete," *International Journal of Social History*, 1, 2 (Autumn, 1956), pp. 253-295.

their specialized competence. Thus Max Weber addressed them:

He who is not in daily contact with source materials . . . is never safe from error in details, and it is therefore quite evident that the final verdict concerning these problems is a matter for historians, archeologists, and philologists, to whom on our part we merely offer for testing the heuristic aids and suggestive questions that derive from our experience as sociological specialists.⁸

Naturally, the writer is more fully aware of the sociological difficulties confronting him than he is of the historical. When he seems to disregard the latter, therefore, the fault may simply be ignorance rather than evasion.

2. NO QUARTER FOR THE PROPHETS

Distinguishing *prophecy* as forecasting in a unique time-series on the concrete level⁹ from *prediction* as "the systematic statement of the probability of the recurrence of phenomena that, for the purposes in hand, are regarded as identical,"¹⁰ it can be said that to pose a predictive problem is to pose a scientific problem. The scientist does not try to prophesy, but he does try to predict.¹¹ He may speak on occasion, especially when dealing with laymen, in the highly controversial terms of cause and effect; nevertheless, his formulations can always be rephrased, insofar as they are genuinely scientific, into probability statements like the following: "If this phenomenon, abstracted as a construct from its unique setting, has happened, is happening, or eventually happens, other things being equal or irrelevant, that phenomenon, similarly abstracted, can be shown to have happened, to be happening, or as likely to happen within

⁸ Max Weber, *Gesammelte Aufsätze zur Sozial- und Wirtschaftsgeschichte*, Tübingen: J. C. B. Mohr (Paul Siebeck), 1924, p. 280 (writer's translation).

⁹ See TVTSI, pp. 94, 97-100, 106-108, 208-213.

¹⁰ TVTSI, pp. 97-102.

¹¹ The problem of prediction is a knotty one, but may be a little easier to handle if sufficient attention is given to distinctions between hypothetical and actual prediction, and between retrospective and prospective prediction. See TVTSI, pp. 284-290.

But, see also the résumé of critiques of Bayes' Theorem, which bears some relation to what is here called retrospective prediction, in R. A. Fisher, *The Design of Experiments*, 6th ed., London: Oliver and Boyd, 1951, pp. 6-7.

certain 'more or less' limits—limits of frequency, intensity, and so on." The "more or less" may not now be specifiable in numerical terms without producing a spuriously precise effect, but whenever such terms can be validly used, they most certainly should be. The goal is as precise statement as possible of functional relations (in the mathematical sense) among constructs.

It follows that research based on this view of prediction is in no sense anti-quantitative. Such research, conducted in a scientific spirit, is at most *non-numerical*: given the character of the evidence and the conceptual apparatus, honesty may demand the statement of findings without invoking the prestige of numbers. It could never be non-quantitative, for "more or less" indicates quantity of *some* kind. While arrayed as a scientist, to tilt against quantity in this broad sense is to be a Don Quixote.¹²

3. NORMS ARE INESCAPABLE

What of social-scientific and, more especially, of sociological prediction? From what we know today of human in contrast to non-human behavior, it seems safe to assume that when a problem relating to the former is posed in predictive terms, it must also be accurately placed in the context of a culture of specifiable scope.¹³ Putting a similar point: personality systems, social systems, and cultural systems cannot be fully telescoped into one another, but they do fit together reasonably well over given ranges, and such fit and range must receive due attention. Cultural systems, in particular, can be viewed as incorporating the norms, explicit or implicit, "external" or "internal," pervading personality systems and social systems.¹⁴ Here description and analysis of

¹² TVTSI, pp. 113, 119, 160, 219-220, 232. It is hoped that the informed reader will view with indulgence the failure to discriminate, in the present space-limited context, between "numerical," "quantitative," etc.

¹³ See footnote 25.

¹⁴ This statement, it will be recognized, summarizes certain aspects of the "general theory of action" formulated by Talcott Parsons (and E. A. Shils) in *Toward a General Theory of Action*, Cambridge, Mass.: Harvard University Press, 1951, Part II. However, unqualified acceptance by the writer of the basic theory should not be inferred; in particular, the equilibrium assumptions, the

cultural systems—which is to say, culture case study—are sociologically indispensable, for they aim at discerning the norms¹⁵ which, themselves manifested in or inferable from behavior, affect the personal and social behaviors in question. "Affect?"—indeed, that have rendered these behaviors human by constituting them social actions in the Weberian sense.¹⁶

To illustrate: What might be expected in the "small-group" conduct of the fifth- and fourth-century B.C. Athenians labeled by themselves and others as Laconizers? The answer requires precise knowledge of the meaning of the label. Laconia, a district in the southern Peloponnesus, was often used to designate what we now loosely call Sparta (Attica and Athens stood in a similar relation.) The Laconizer was a person trying to pattern his social actions in accordance with the norms of the rulers of the Spartan domain, usually called Spartiates. Nevertheless, their imitators took the name of the region regarded as most typical of Spartiate control, namely Laconia. There the Serfs and Satellites,¹⁷ together with many erstwhile Spartiates whose property was insufficient to keep them among the rulers, were held in subjection.

Consequently, the Athenian termed a Laconizer was an avowed admirer of Sparti-

functionalism, and the Freudianism embodied in the theory do not seem tenable. What is here "accepted" is merely the long-established treatment of personality systems, social systems, and cultural systems as not only analytically separable, but also as mutually irreducible.

¹⁵ Culture case study, *in explicit form*, is not the only way of discerning norms. Simple "knowledge by acquaintance" may carry the investigator a long way—but when such knowledge is reliable, it is based on *implicit* observation and inference that essentially amount to culture case study. When someone asserts that good research involves "knowing the universe" to be analyzed, he is actually proclaiming that culture case study of at least implicit character should be conducted before extensive commitment to the technical operations *per se*.

¹⁶ This sense has been current at least since Theodore Abel, *Systematic Sociology in Germany*, New York: Columbia University Press, 1929, Chapter 4; and WBSS (1932), pp. 53-62 and Part Two, "Systematics of Action Patterns."

¹⁷ Convenient English renderings of *heilôtes* and *perioikoi*. The writer here asks pardon for the introduction of his own special terms, especially as there is no space available to make their meaning less cryptic.

ate prescriptive oligarchy,¹⁸ sacred in many respects, as opposed to Athenian principia¹⁹ democracy,²⁰ with its numerous secular features. As emblematic of his admiration, he displayed his hair at shoulder length (bleaching it if necessary), fingered a mustacheless beard, boxed at least enough to acquire a cauliflower ear or a thickened nose, pranced along in red shoes, draped himself in a scarlet cloak, sported a cane with the appropriately elegant curve, ostentatiously consumed black broth (highly distasteful to Athenian palates), led on leash a Spartan fox-hound, and affected choppy conversation sprinkled with Doric words—which is to say, talked Laconically.²¹

The Laconizing club to which this Athenian belonged was one among many such semi-conspiratorial groups, with vows, as Plato hinted, "to do the sweaty, greasy democracy of the Piraeus all the damage possible," and in addition with homosexual obligations. When he married, he induced his bride, as far as he dared, secretly to garb and otherwise comport herself like a young male, and in any case continued his homosexual attachments. These were by no means unknown among non- or anti-Laconic Athenians, but adverse moralistic judgments were nevertheless likely to be passed on his conduct in many circles, whereas in Laconia, and especially among the Spartiates, he would have evoked little or no comment²² as long as he concealed the fact that he was actually, for all his Laconism, only a despised outsider.

Without using a culture case study or its equivalent to lay bare these normative patterns, how would it be possible to make pre-

¹⁸ MSTICAC, pp. 154, 166-67.

¹⁹ MSTICAC, pp. 155-157, 169-170.

²⁰ An excellent survey of the sources from which much of this evidence is drawn is W. A. Becker, *Charicles*, translated by Frederick Metcalfe, 6th ed., London: Longmans Green, 1882, esp. pp. 63, note 8, 419. For some purposes, however, recourse to the Göll edition (see note 21) is advisable.

²¹ Erich Bethe, *Tausend Jahre altgriechischen Lebens*, München: Bruckmann, 1933, pp. 21-41, which draws on several of his earlier articles on Dorian pederasty, etc.

See also W. A. Becker, *Charikles*, neu bearbeitet von Hermann Göll, Berlin: Calverly, 1876, Vol. II, 2. *Exkurs zur 5. Szene (by Göll)*, "Die Knabelliebe," pp. 252-285. This should not be confused with the bowdlerized Metcalfe translation.

dictive statements with reference to the "small group" life of Laconizers that would have even remote prospects of validation? We must "know the universe" with which we deal—and this holds for "contemporary" as well as "historical" problems. Certainly we would not deal with gangs, today's or yesterday's, without detailed information about the normative context in which this or that gang activity runs its course.²³

4. DRAG THE ASSUMPTIONS INTO THE OPEN

But we are getting ahead of the story. Clearly, any investigation starts with a problem, explicitly stated or not. This may arise merely because the investigator is perplexed by the fact that the answer to his particular question is not immediately obvious, or it may transcend the particular because of the discernible bearing that it has on crucial scientific issues. For the sociologist, scientifically relevant problems arise in connection with his knowledge of crucial differentials in human behavior that no amount of information about man as an organism can account for adequately.²⁴

Using his prior knowledge of social actions as empirical²⁴ ground for assumption, he structures his initial question so that the differentials are at least implicitly stressed. In other words, he assumes, with empirical warrant, that a significant proportion of the problematic variations in the personality and social systems he proposes to examine will ultimately be assignable to

²² Thrasher's study of gangs falls short of maximum utility because of failure to focus on normative patterns. "Ecological" theory, with stress on "interstitial areas," was responsible.

²³ The sociologist can most conveniently deal with "biologically standard" human beings. There is no intent cavalierly to dismiss the problems involved, much less to perpetrate "sociologism" in whatever form. See TVTSI, pp. 19-20, note 21.

Moreover, there is no dogmatic insistence on the old split between the natural sciences and the "mental" sciences (*Natur- und Geisteswissenschaften*). Did space permit, however, the point would be made at some length that the distinction between the natural sciences and the cultural sciences (*Natur- und Kulturwissenschaften*), as set forth by Rickert, Znaniecki, and others, is quite relevant. This distinction does not necessarily involve metaphysical dualism.

²⁴ Note the frequent repetition of "empirical" in this paragraph. The purpose is to emphasize the down-to-earth character of the theory involved.

the variations that the concomitant cultural system has developed²⁵—and under certain circumstances, of course, *vice versa*. Further, he assumes, again on an empirical basis, that the cultural system he studies is in one crucial respect a normative system, and that the problematic variations in social actions are therefore predictively linked with normative variations.²⁶ Still further, he assumes, given known empirical evidence, that the normative system in question is a system, however constituted, and hence that when variations in some of its components are observed, variations in others are likely to be discovered. Once more, he assumes, remaining within empirical limits, that the links between variations are flexible in all their combinations, which is to say, dropping the metaphor, that any predictions about the relationships of social actions, of norms with norms, and of social actions with norms must be expressed in terms of probability.²⁷ Cul-

²⁵ This is not a blank check for cultural determinism; "a significant proportion of the problematic variations" is sufficiently limited to leave an appropriate balance for other assignments.

²⁶ ". . . the major contributions of . . . Sumner—the relativity of culture and the still incompletely appreciated fact of its affective basis and its permeation with sanction and moral values"—G. P. Murdock, *Social Structure*, New York: Macmillan, 1949, p. xii.

²⁷ The writer has long stressed "objective probability" in opposition to "objective possibility" or mere plausibility, as witness TVTSI, pp. 97, 108, 172–173, especially note 92, 195, 261–262, 282. Recently he has been gratified to see that, although most followers of Max Weber have rested content with interpretative plausibility, proper attention to Weber's occasional stress on rigorous validation has at last been granted. See Werner Ziegenfuss, editor, *Handbuch der Soziologie*, Stuttgart: Ferdinand Enke, 1955, Vol. I, p. 215, where he cites the following passage from Weber:

"Meaningful" interpretations of concrete behavior *per se* are of course . . . only hypotheses of imputation. They therefore stand in need of the most rigorous validation by means of the same procedures, in principle, as any other hypotheses. They are usable hypotheses for us when and if we may assume that a suitable degree of probability (greatly varying from case to case) of subjectively "meaningful" motivational interconnections is present. Causal connections with which motivations oriented toward purposive rationality are interwoven are, under certain favorable circumstances, especially with regard to just such rationality, directly susceptible of statistical test and therefore, in such cases, capable of being proved valid (in a relatively optimal way) as "explanations"—*Gesammelte Aufsätze zur Wissenschaftslehre*, Tübingen: J. C. B. Mohr

ture case study therefore does not start in a vacuum; it cannot be carried out properly without making at least indirect use of attested sociological principles.

Referring specifically to Greek history, one might ask, as the writer did some years ago, "How did it come about that secularization proceeded with such apparent rapidity in 'Athens' during the fifth and fourth centuries?" Before this question arose, it should be stressed, the key term, secularization, had begun to acquire fairly definite meaning to him.²⁸ Moreover, as theoretical differentiation and expansion, and other research on secularization, have progressed, it has become evident that the several assumptions discussed above were implicit in that question. No research ever starts without assumptions.²⁹ Whenever possible,³⁰ they should be empirically demonstrable; luckily, the more immediately relevant assumptions were so in the Athenian case.

5. KNITTED BROWS, SELECTED WHOLES, AND NOT-WHOLES

How the question arose is another matter. Perplexities growing out of the writer's somewhat casual reading of Greek history, and having little immediate bearing on key sociological issues, doubtless were important initially. Such uneasiness often leads nowhere, but in this instance theoretical orientations acquired during prior sociological

(Paul Siebeck), 1922, p. 413, translation the present writer's.

²⁸ Cf. MSTICAC, pp. 176–184.

²⁹ It is interesting to listen to Pure-Induction advocates who invoke the name of Darwin as the great Freedom-from-Theory apostle. But the following passage seems important:

In October 1838, that is, fifteen months after I had begun my systematic inquiry, I happened to read for amusement *Malthus on Population*, and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favorable variations would tend to be preserved, and unfavorable ones to be destroyed. The result of this would be the formation of new species. *Here, then, I had at last got a theory by which to work*—Francis Darwin, *The Life and Letters of Charles Darwin*, New York: D. Appleton, 1919, Vol. I, p. 68, italics the present writer's.

³⁰ Cf. Abraham Wolf, *Essentials of Scientific Method*, 2nd ed., New York: Macmillan, 1937, Chapters 5 and 6, and note G, pp. 170–171.

training gave guidance; therefore, the Greek perplexities were a welcome challenge. Furthermore, suggestions and ensuing guesses about the tentative answer perhaps had a good deal to do with the shape that the question soon took. Questions may emerge, then, in many ways; there are no rigid rules for generating them. "Logical" is one thing, "psychological" another. And here the temptation to anticipate must be resisted.

Resisted, because the major reason for referring to the question at this point is to underscore the fact that any culture case study should be explicitly selective, and that the nature of the question posed fixes at least the initial limits of selection, which is to say of relevance. Otherwise put, even a preliminary research "whole" is a whole by interrogatory definition.

"Athens during the fifth and fourth centuries" represents a dated and localized whole;³¹ occurrences in Greek history or even prehistory falling outside its confines may prove relevant, but such relevance must be demonstrated. Moreover, not all events falling inside are necessarily relevant, for the question has to do with the *secularization* of the society. Further, the question is so phrased that the slower kinds of secularization, often counterbalanced by sacralization,³² that go on in all societies are not of primary concern; it is the *apparent rapidity* of secularization that sets some of the limits of selection.³³

Those limits must not be unduly rigid. As research proceeds, the scope of the whole alters as the researcher's definition changes in accordance with newly discovered facts, familiar facts seen in fresh perspective, improved selection, shifts in the concomitantly

³¹ Obviously, knowledge of and practices deriving from other societies may enter into such a whole; certain aspects of Spartan life came to be incorporated, albeit with important alterations, in Athenian life. This underscores the basic point that the whole under consideration is a whole by definition.

³² MSTICAC, pp. 154, 166-168, and especially 173-176.

³³ This "apparent rapidity" is of course one of the matters later to be proved or disproved. There was selection by the writers of the initial documents—indeed, the documents were written, in some cases, because of the "apparent rapidity." All this means that the evidence must be handled *very* carefully.

developing theory, and the like. Nevertheless, the whole at any given time is a whole by interrogatory definition.

Trained researchers of course know the impossibility of gathering "all the facts" about anything.³⁴ But the exponent of culture case study refrains from trying, for quite explicit reasons. He wants only relevant facts, and the question he asks determines what is likely to be relevant. "Wholes by intuition" are not for him,³⁵ his concern is with "wholes by interrogation."

A culture case study, then, is fundamentally different from a case record, therapeutic or otherwise, a catalog, a chronicle, a this-followed-that narrative, or a supposedly given whole to be placed in relation to other such givens or encompassed within some all-inclusive entity.³⁶ It is a selective description of relatively unique societal phenomena designed to return a tentative answer to a question about the relationships of social actions and norms that, in principle at least, admits of predictive solution. It may draw on materials that "history" provides, or on "contemporary" evidence, or both. Even when limited to the "historical" by the scope of the question posed, however, it is not history as such, but a sociological study of history.

6. WHO SAYS WHAT'S WHAT?

Once the question has been formulated and the underlying assumptions surveyed, the next step is substantive research within

³⁴ TVTSI, pp. 98-100.

³⁵ Ruth Benedict's *Patterns of Culture*, New York: Houghton Mifflin, 1934, is a cardinal example of the selecting of "wholes by intuition." Still, it is evident that there was an implicit, vague question somewhere in the background—perhaps a question deriving from the Apollonian-Dionysian contrast. Had the question been explicitly stated, and had other procedural safeguards been set up, the study might have achieved more than it did. As matters finally stood, however, the results did not go beyond: (1) "primitive" illustration of supposed "classical" contrasts; (2) description of esthetically apprehended wholes; (3) and relativistic preaching about the desirability of "appreciating" value-systems other than our own. There was no validation worth mentioning. See the discussion by A. E. Goldenweiser in H. E. Barnes, Howard Becker and Frances Bennett Becker, editors, *Contemporary Social Theory*, New York: D. Appleton-Century, 1940, pp. 482-489.

³⁶ Mandelbaum, *op. cit.*, pp. 1-14 *et passim*.

the interrogatively relevant whole. In such research, there arises at once an important issue: Whose definitions of situations are to be taken as predictively fundamental? The answer is implicit in the well-worn phrase, "If men define situations as real, they are real in their consequences." The persons whose actions are under investigation are those who say what's what, as do the Polish peasants of Thomas and Znaniecki. But do they always say it, and is what they say, when they do, the predictively fundamental what?

Taking one thing at a time, it is clear that they do not always say it. Many situations are defined in unverbalized ways: by frowns, warning gestures, tense postures, significant silences, tunes lightly hummed, and so on.³⁷ The user of historical records (and here "records" includes everything from trumpets to puns)³⁸ is in some respects handicapped when dealing with such unverbalized definitions, but for Greek life, at least, the graphic and plastic arts represent a great asset. The vase-paintings, in particular, are extremely valuable, for in them many quite ordinary social actions are faithfully represented. When Patroclus has his wounded arm bandaged, it can be seen that grimaces of pain were not always regarded as beneath the dignity of the aristocratic warrior. When Zeus wanders through the streets by night with a ladder, glancing toward the upper window through which a lovely damsel is peering with face immodestly unveiled, it can be seen that Athenians in the very highest levels of the social hierarchy may have lost little standing by engaging in amorous escapades. When foreign traders are caricatured as over-dressed, hook-nosed, flat-footed, and flabby, it can be seen that even when they are shown offering for sale luxury goods in great demand, they seem to be viewed with amused contempt.³⁹ So might

the evidence from graphic and plastic sources be multiplied; unverbalized definitions are abundantly represented.⁴⁰

When attention is turned to what the subjects do say, the researcher has a wealth of material. True, he must know some Greek; in spite of centuries of critical scholarship, commentary, and translation,⁴¹ ability to work with primary sources or, at least, safely to draw on the conclusions of other scholars, is a *sine qua non*. Linguistic sophistication is necessary, however, not only in "historical" investigation generally, but also in the study of "contemporary" matters. The argots of criminals, teenagers, executives, and craftsmen, for example, often require careful interpretation; the meaning for the outsider is not always the meaning for the initiate.

The user of historical materials has one great advantage over both the ethnographer confronting peoples without writing and the sociographer⁴² who neglects available records, for he can discover how definitions of situations change over time. When he finds that *nemesis* once meant only "the righteous indignation of men," in a context where "men" referred essentially to equals, as over against a later period when *Nemesis* became a goddess or a holy force charged with visiting future retribution on those exploiting the helpless, he has a clue to shifts in definition that accompany shifts in social stratification. The deviant in a war-band of "companions" feared the adverse normative sanctions of his fellows; their *nemesis* had grim and speedy consequences. By contrast, the over-

always possible) offers adequate safeguard. Moreover, those later interpreting the work of the artist may impute meanings that he did not intend. The writer has carefully weighed, however, the question of using strong terms such as "grimaces of pain," "amused contempt," etc., and has retained them because he feels that they are warranted. When studied alongside the more restrained treatments, the imputations seem by no means extreme.

³⁷ Such definitions play highly important parts in what the writer has recently termed "proverbial societies"—MSTICAC, pp. 152-153, 165-166.

³⁸

Scope so inclusive carries the conception of "historical record" far beyond "document" in any ordinary sense.

³⁹ It should be noted, of course, that artists are not to be entirely trusted where unverbalized definitions are concerned. There is always a certain amount of conventionalization; only critical comparison of various representations (and this is not

⁴⁰ For the ceramics, mention need be made only of the stupendous *Corpus vasorum antiquorum*.

⁴¹ The great amount of work done in past centuries on the societies of Greece and Rome, some of it still without need of major revision, should be of much interest to social scientists, whatever their specialties. See the impressive article on "Scholarship in Modern Times" in the *Oxford Classical Dictionary* (1949).

⁴² This is a term coined by S. R. Steinmetz, the Dutch sociologist.

lord wresting tribute from peasants who three centuries earlier would have been socially defined as his "companions," had little apprehension when the downtrodden invoked the remote reprisal of the unseen, *Nemesis*.⁴³

Returning to the moot point, in culture case study every effort must be made to specify the definition of the situation offered by the subject or subjects, without initial admixture of the observer's definition. As the writer has put it elsewhere:

The social scientist knows that he cannot proceed as though the persons he happens to be studying are responding to a situation as defined by *him*. . . . He must not first of all select and trim his units by what he as a scientific observer responds to, but by what his subjects selectively define and hence make effective as units in the structure of their actions.⁴⁴

Social actions appear in clusters, to many (although probably never all) of which the subject gives names: clan, tribe, city-state, Laconizers, war-band of companions, repressive secret society (*Krypteia*), traders' guild. In other words, he constructs what, for the observer, are at least preliminary abstractions. Norms, which include abstractions from perceivable social actions and inferences about them from other actions, similarly manifest clustering—indeed, they are in a sense only patterned aspects of social actions having various sacred or secular implications.⁴⁵ Here too the subject applies names: harmony, piety, treason, normlessness (*anomia*), and so on.

Ready-made definitions of situations, therefore, are encountered on every hand when conducting culture case study. Further, they are not merely culled from a lexicon, for they occur in what is frequently a very full context that leaves little doubt as to what those holding such definitions actually did under their presumed guidance. Thus the social actions of Socrates were defined as

⁴³ Hesiod, *Works and Days*, 126 *et seq.*; M. P. Nilsson, *Homer and Mycenae*, London: Methuen, 1933, p. 114; Gilbert Murray, *The Rise of the Greek Epic*, 3rd ed., Oxford: Clarendon, 1924, pp. 51, 81-82.

⁴⁴ "Science, Culture, and Society," *Philosophy of Science*, 19, 4 (October, 1952), pp. 279-280. Some American social psychologists, shifting Husserl's meaning to a certain extent, have recently used "phenomenological" to designate this point.

⁴⁵ MSTICAC, pp. 142-161.

impious (as amounting to *asebeia*) by the simple-minded and well-meaning democrats who accused him; the fatal outcome, given the Athens of that day, could readily have been predicted—in fact, it was so predicted—by most of the participants. Moreover, subjects may apply normative abstractions to their own actions, in the manner of Xenophanes, Solon, Socrates, and other Greek rationalists, in effect declaring: "I do as I do, and will continue to do so, because I hold that reason is rightfully supreme in all things, and I make every effort to follow its dictates."

To this extent, then, the definitions of situations, abstract in varying degree, as presented by subjects are viewed by the practitioner of culture case study as essential evidence. The units to which the subject responds must be determinably the subject's units; his definitions, unverbalized and verbalized, of these units and their appropriate responses must be kept as separate as possible from the observer's definitions.⁴⁶

To be sure, subjects, for various reasons, may be in error. Ready-made definitions cannot be accepted without question, for they may not be predictively valid. Even when unverbalized they may be vague, ambivalent, or unduly restricted in scope, and on occasion deceitful. What men say they do and what they actually do are often in startling contrast, and among the most startled, in some cases, are the men themselves. Once more, when a subject makes his abstractions on the basis of limited experience, a survey of similar abstractions made by other subjects often yield predictive probabilities unforeseen by either the limited-experience subject himself or by the observer relying only on him.

Nevertheless, variations among social action clusters and normative patterns would make predictive efforts little more than shots in the dark if the utmost pains were not taken

⁴⁶ Much reporting, even when carried out by social scientists, lacks precision. There is little adequate separation between what the subject actually said or otherwise symbolized, and what the observer says the subject said. In many instances, indeed, there is no effort to report anything *verbatim*, and even when there is, no chronological order is followed. Further, "striking" *verbatim* fragments often give a deceptive impression, and sometimes only such fragments are quoted.

to insure that situations are defined as subjects *effectively* define them. Such effective definition of course is not always the same as the unwary researcher might infer from the available collection of unverbalized and verbalized definitions. Without the clues that the latter afford, however, research would proceed at random or, possibly worse, along the lines of loose analogy based upon supposedly similar wholes.

It was stressed above that the researcher's definitions should be separated to the extent possible from those of the subject. It is all too easy to overestimate such separation; from what the subject selects the observer in turn selects. Conscientious observers try to refrain from "forcing the evidence," but even they cannot avoid their own peculiar kind of abstraction. The writer recently phrased the matter thus:

[The social-scientific observer, even when "sophisticated"] . . . wittingly or unwittingly imposes normative judgments on what is observed . . . *qua* scientist, his norms lead to the exclusion of esthetic, playful, religious, and like considerations except as they promise to yield greater predictive power.⁴⁷

Thus to yield allegiance to prediction is eminently proper in the final phase of culture case study formulation, but in the earlier phases the observer, proceeding from the subject's abstractions, must try to envisage what happens when "esthetic, playful, religious, and like considerations" are the basis of normative judgments. In other words, he must try to duplicate the knowing, desiring, and choosing processes characteristic of the subject. He need not be Hesiod in order to understand Hesiod,⁴⁸ but he must have attempted to deal with Hesiod's definitions as such without first insistently asking, "What can I now predict about Hesiodic conduct?" When *Hesiodic* social actions and normative abstractions are clearly evidenced, only then should the observer offer his interpretation (so labeling it) and venture his predictive judgments.

In sum, the observer must ultimately say what's what, but he can do so only when he is fully aware of what's what for the subject.

⁴⁷ MSTICAC, p. 141; cf. TVTSI, pp. 281-285, 290-297.

⁴⁸ Cf. Max Weber, *op. cit.*, p. 408.

7. THE QUESTION IS ANSWERED WITH FINGERS CROSSED

When clusters of social actions and norms are extensive, intricately interrelated, and difficult for the observer to enter into vicariously to a degree sufficient for unforced interpretation in brief compass, culture case study may be quite lengthy. Extensive reference to and even quotation of original sources are often necessary, but in some instances can be avoided if the reader's reasonable familiarity with them can safely be assumed, or if the writer is as skillful in condensed paraphrasing without distortion as Gernet, Glotz, or Schuhl.⁴⁹ Sometimes relative brevity can be achieved by tabular presentation and numerical summary, as in the case of Dutch and German research on such central Greek norms as restraint through anticipated shame (*aidōs*), insen-sate pride (*atē*), established sanction (*themis*), and the holy (*hagios*).⁵⁰

Whatever the form of exposition, the focus must be on the relevance-determining question. Eventually an answer, always tentative in the culture case study proper, must be found. In the present example, the question about Athenian secularization has been answered by the writer thus: "It was rapid, in comparison with the period immediately preceding, *apparently* because of the indigenous development in Athens of hitherto exogenous value-systems, Laconism and Ionism, that manifested strongly secularizing tendencies in the societies where they arose, and that had even greater secularizing effect when brought into conflict with each other and with the earlier Athenian value-system in a setting inclusive of all three." More briefly, three normative systems, each previously isolated, seem to have clashed with suddenly secularizing consequences in late fifth- and early fourth-century Athens.

⁴⁹ Louis Gernet, *Recherches sur le développement de la pensée juridique et morale en Grèce*, Paris: Ernest Leroux, 1917; Gustave Glotz, *La Solidarité de la famille dans le droit criminel en Grèce*, Paris: A. Fontemoing, 1904; Pierre-Maxime Schuhl, *Essai sur la formation de la pensée grecque*, Paris: Alcan, 1934.

⁵⁰ For a list of some of the important works, see the *Oxford Classical Dictionary* (1949), "Religion, Terms Relating To," p. 758. The treatise by J. C. Bolkestein is especially valuable.

Here, the stress on procedure limits the importance of the answer's effect on our view of Greece; it serves an illustrative purpose only. What is significant, procedurally, are the qualifications (in italics): "apparently" and "seem to have." Any answer to culture case study as such is tentative, for there has been no validation that will stand up under scrutiny. The product of a culture case study is a preliminary answer, perhaps serving as the source of a definite hypothesis. Hypotheses must be proved or disproved.

It has been said that case studies in general function only to generate "hunches." This is both over- and under-statement. *Overstatement*, because many case studies are merely rambling descriptions of one or another "whole by intuition," not focused on clearly formulated questions, and usually regarded by their authors as sufficient unto themselves. They sometimes stimulate hunches, but readers knowing a little about the world and its ways probably have had such hunches many times before. Moreover, miscellaneous hunches are little better than none at all: random search is not research. *Understatement*, because when a good culture case study serves to generate a tentative answer, it is worth following up. Hunch it may be, but not a random one. A definite hypothesis can be formulated on the ground it delimits, which can be submitted to the tests for proof or disproof. To make this possible is to do a good deal.

8. IT ISN'T ENOUGH TO BE ON THE LEVEL

What about the traditional idiographic-nomothetic distinction?⁵¹ The distinction should not be discarded, but discretion should be exercised in its use. For it designates, in fact, poles of a continuum: from the *relatively* dated and localized to the *relatively* undated and nonlocalized.⁵² By adding particulars, the continuum can be idiographically extended; by making increasingly general statements, it can be nomothetically lengthened. Precisely what

⁵¹ Idiographic (not to be confused with ideo-graphic) has meant, ever since Windelband and Rickert, "pertaining to the description of the unique." Nomothetic, on the other hand, means "pertaining to established law" (law in the scientific rather than the legal sense).

⁵² TVTSI, pp. 105-107.

the range should be depends on what the problems are that use of the continuum is to simplify.

Culture case study is idiographic, but the assumptions, the *intellectual* operations, an intrinsic part of the procedure, are nomothetic. As noted above, there must be "at least indirect use of attested sociological principles." Moreover, the goal of the procedure is toward the nomothetic pole. As sociologists, we must try to generalize.

The problems set by us have to do with the varying relationships of social actions and norms, and the combinations of these occurring within interrogatively defined wholes must be described in whatever fullness of detail is necessary for the development of tentative answers. Hence, such descriptions as well as the tentative answers are idiographic. Both the question and answer about Athenian secularization, for example, are in the realm of the particular. To be able to say how Athenian secularization may have come about does not carry the researcher beyond *Athenian* secularization. The hunch, although derived from extensive relevant evidence, not only lacks validation, but can lead no further when validated on its own level than an idiographic conclusion: Athens is still Athens. But "when validated on its own level" poses a crucial question.

9. HYPOTHESIS TO THE RESCUE

The validation of the tentative answer requires the setting up of a hypothesis. In a properly designed and executed culture case study, the answer is *always* on the same level of abstraction as the initial question, but *never* the hypothesis. In historical research, the hypothesis is always on the same level as the tentative answer and, obviously, as the question.

This comparison should not be taken to mean that historians customarily proceed in such ways that question, relevant whole, assumptions, answer, and hypothesis are distinguished or, at times, even distinguishable: their objectives are not sociological.⁵³ As

⁵³ To discuss these objectives here would take space that cannot be spared. Cf. Mandelbaum, *op. cit.*; Benedetto Croce, *On History*, New York: Harcourt Brace, 1923; and TVTSI, pp. 208-213.

suming, however, that in strictly historical monographs hypotheses of some kind can be discovered, they are designed only to show, by examination of evidence not included in the first part of the account or not viewed from the same perspective, that the particular items surveyed earlier belong together in a unique whole that can be fitted into progressively larger unique wholes. Athens remains peculiarly Athens, but its peculiarity can be better depicted by accurately placing it in relation to the unique and more inclusive Hellenistic world, and this in turn to the unique and still more inclusive Roman world, and so on.

The sociologist conceives his task differently. His validation must be predictive—albeit, in “historical sociology,” in the retrospective sense. He must say, “If and when these combinations of these social actions and norms recur, in forms that can be regarded as identical for the generalizing purposes in hand, in situations likewise identical, then these are the probable consequences.”

For the historian nothing recurs: “history as history does not repeat itself.” For the sociologist everything in the human realm with which he is concerned recurs if the level of abstraction is high enough; “sacralization and secularization appear at all times and places.” Clearly, the level may be so high (as in this illustration) as to prompt “So what?”—even by well-wishers. For sociologists must try to generalize, but they may be so excessively general—so far over on the nomothetic side of the continuum—that they lay themselves open to severe criticism.⁵⁴ Generalization is always omission, and we often omit too much. Consequently, framing a hypothesis is a demanding business; it should not lead to the mere prophecy of the news commentator, on the one hand, nor to prediction so abstract, on the other, that almost any event can be presented as “conclusive” evidence.

A suitable hypothesis, however, serves to guide the researcher safely between the extremely idiographic and nomothetic poles. One designed to advance research on the Athenian problem has thus been shaped by the writer: “If and when value-systems

⁵⁴ Sometimes, it must be granted, the criticism is not merited. See TVTSI, pp. 119–124.

suites to societies of prescriptive type maintained primarily by coercion, and value-systems suited to societies of pronormless type rejecting coercion, come into sudden conflict with each other and with value-systems suited to societies of principal type that make only restricted use of coercion and stress their situational accessibility, this conflict and its consequences within the confines of such principal societies then have a high probability of rapidly secularizing, in extreme degree, the latter’s value-systems.”⁵⁵ Schematically, “If P, then Q.”⁵⁶

Here, the hunch or tentative answer has been partially stripped of its idiographic particularities. But its formalization and use as a hypothesis must go beyond what might seem to be terminological jugglery and potential “generalization by omission.” A hypothesis must be validated, which means that evidence differing in content or in perspective than that from which the tentative answer was derived must be submitted and subjected to the appropriate canons of proof and disproof, including whenever possible, those of quantification. The necessity of validation is indicated in the formulation above by the words and phrases “if and when,” “suited,” “type,” “consequences,” “then,” “high probability,” and “in extreme degree.”

It must be emphasized that when the level of abstraction is raised, assumptions are necessarily built into the hypothesis. These, like the assumptions in the initial question, should have firm empirical foundations or, at a minimum, should stem from sociological theory that has or could have been derived from available empirical evidence. The assumptions of the hypothesis presented above, the writer believes, have been utilized in sound and reciprocally related sociological research and theory, and are in accord with attested and empirically grounded sociological principles.

Specifically, these assumptions refer to the nature of proverbial, prescriptive, principal, and pronormless societies, of their corresponding value-systems, and of sacralization

⁵⁵ This hypothesis necessarily contains somewhat unfamiliar terms. The writer believes, however, that he is not indulging in mere terminological gymnastics. For reasonably full specification of the referents involved, see MSTICAC, pp. 133–176.

⁵⁶ TVTSI, pp. 259–279.

and secularization; they are necessarily well over toward the nomothetic pole of the continuum. The hypothesis presents these assumptions in combination appropriate to the tentative answer, and includes others, implicitly and explicitly, appropriate both to the combination and the tentative answer. And the tentative answer, it will be remembered, was developed through the close scrutiny of a "whole by interrogation." Fact implicates theory; theory implicates fact.

In this reciprocal process the tentative answer has been transcended; in formulating a hypothesis to be tested, there has been a shift from the idiographic toward the nomothetic. "When validated on its own level" can be stricken out. But how is the hypothesis to be tested?

10. TYPES AID HYPOTHESES

Constructive typology has begun to receive the renewed attention that its procedural merits would seem to require,⁵⁷ but even now there is insufficient realization of its close linkage with culture case study in both "historical" and "contemporary" contexts. Likewise lacking is full understanding of the fact that hypotheses, at least in the social sciences, often incorporate types as essential components. (To be sure, it has also been argued that types incorporate hypotheses—which is the case in only a special and limited sense.)⁵⁸

A type is constructed of criteria that have discoverable referents in the empirical materials relevant to the initial question with which the culture case study got under way.

⁵⁷ Especially by J. C. McKinney; see his chapter in Doby, *op. cit.* Also important is the article by C. G. Hempel, "Problems of Concept and Theory Formation in the Social Sciences," Symposium Proceedings, American Philosophical Association, Eastern Division, published in *Science, Language, and Human Rights*, Vol. I, Philadelphia: University of Pennsylvania Press, 1952, pp. 65-83. The writer feels that Hempel has not fully understood constructive typology, but nevertheless welcomes the searching critique that he provides.

Space is here lacking for presentation of constructive typology as such. This article is to that extent a torso only. The best that can be done is to refer again to McKinney's excellent treatment in the Doby volume, and to Chapters 2, 4, and 5 of TVTSI.

⁵⁸ Hempel, *op. cit.*, p. 78. Cf. TVTSI, pp. 109, 113, 123, 219-220 (especially note 57), 232, 262, 265.

Any type that is usable in the testing of a hypothesis derives from, first, a hunch that in turn derives from, second, a culture case study itself deriving from, finally, the social actions and norms surveyed in the study or others akin to it. Unless type criteria are drawn from the culture case study in conjunction with which they are used, as well as from other such studies providing the empirical bases for the assumptions embodied in the problem-setting question and the hypothesis, they are apt to be based upon untrustworthy views.⁵⁹

As noted above, hypothesis and type may be closely linked, but in a specific culture case study, they are clearly distinguishable and should never be confused. "P" and "Q" are the substantive and/or processual types;⁶⁰ when they are explicitly related by "if" and "then"—"if P, then Q"—the resulting formulation constitutes the hypothesis. When they are not so related, they are only potentially hypothetical.

Manifestly, types must be used in the culture case study proper, for the social actions and norms grouped together and conveniently labeled for the purposes in hand are always unique.⁶¹ For example, it would

⁵⁹ For illustration of the lengths to which, e.g., the organismic analogy has been pushed, see the following articles by R. W. Gerard: "Organism, Society and Science," *The Scientific Monthly*, 50 (1940), pp. 340-350, 403-412, 530-535; "A Biologist's View of Society," mimeographed lecture in "Conflict and Cooperation" series, University of Chicago, Spring, 1945; "Higher Levels of Integration," *Biological Symposia*, 8 (1942), pp. 67-86; "Biological and Cultural Evolution," *Behavioral Science*, 1, 1 (January, 1956), pp. 6-33; "Levels of Organization," *Main Currents in Modern Thought* (May, 1956), no page nos. in reprint; "Units and Concepts of Biology," *Science*, 125, 324-32 (March 8, 1957), pp. 429-433.

⁶⁰ For many purposes the distinction between the substantive and the processual (here in the meanings, essentially, of noun and verb) is unimportant, more especially in view of the fact that substance can always be resolved into process. Cf. WBSS, pp. 23-40.

⁶¹ As examples of this, and of the substantive-processual distinction, the following may be noted: Athenian clans, craft guilds, groups of fellow-celebrants (*thiasoi*) and rite-participants (*orgeones*), Pythagorean brotherhoods, and oligarchic cliques are substantive and markedly idiographic types; prescriptive, principial, and pronormless societies are substantive and strongly nomothetic types. Laconizing, Medizing, and democratizing (in the Athenian sense) are idiographic processual types; sacraliza-

be impossible to describe all of the day-to-day variations in the phenomena designated by the term "city-state," and yet they must be treated as a constant in some idiographic contexts. Types used in the culture case study itself, then, are in considerable degree idiographic, but they are still types.⁶²

In the sociological hypothesis, however, nomothetic types prevail by deliberate design. And all relevant practices of typological procedure may be used.

11. SOME WAYS TO SKIN THE CAT

These practices must of course conform to accepted standards of "causal inference" or, methodologically preferable, of conditional and probabilistic prediction. These standards permit, legitimately, many varieties of such inference or prediction—concomitant variation, and so on. Here attention is given to only an example or two directly related to the validation of the hypothesis presented above.

It would be legitimate, in the writer's estimation, to treat the evidence out of which the tentative answer was derived from a different perspective. In presenting predictions utilizing nomothetic types of societies, the effort would then be to show, by re-examination of the phenomena that originally elicited the hunches, that the idiographic types used in their designation actually

tion, secularization, and so on are nomothetic processual types.

For further detail about idiographic types see the writer's "In Defense of Morgan's 'Grecian Gens,'" note 7 above, and E. L. Minar, *Early Pythagorean Politics in Practice and Theory*, Baltimore: Waverly Press, 1942.

⁶² A clear statement on this point is provided by Alexander von Schelting, *Max Weber's Wissenschaftslehre*, Tübingen: J. C. B. Mohr (Paul Siebeck), 1934, pp. 325-342. It is in some ways paradoxical to refer to "idiographic types;" any type is in certain respects at least potentially nomothetic. To be remembered, however, is the fact that the idiographic-nomothetic distinction "designates, in fact, the poles of a continuum; from the relatively dated and localized to the relatively undated and non-localized" (p. 498 of this article). Consequently, "idiographic type" simply means one that is dated and localized with fair specificity; e.g., "The typical member of the Athenian *kaloi kagathoi* immediately before the First Peloponnesian War." Less idiographic—or, what amounts to the same thing, more nomothetic—would be "the typical Athenian during the 5th century B.C."

represent sub-types of the nomothetic, and that the consequences predicted from the combination of the nomothetic types came about "historically," in fact, in the parallel combination of the idiographic types. Such validation is satisfactory, however, only if the nomothetic types themselves and their predictive combination are already validated by the use of evidence not drawn from the Athenian whole. In other words, the hunch is confirmed only when it corresponds to predictable regularities (*Gesetzmässigkeiten*) that themselves have been adequately validated on other grounds. The social sciences at present can refer to only a few such predictable regularities.

Another handicap appears in *subtyping*. This is legitimate up to a certain point, but it can be so detailed that it constitutes little more than rephrasing of the culture case study in persuasively technical terms. The effort to show correspondence between nomothetic and idiographic types merely takes the form of a terminological exercise. Comparison becomes impossible. Both questions of the *degree* of correspondence and of the "negative utility" of the nomothetic type are disregarded.

A different way to go about the task of validation would be to examine fresh evidence. For instance, the degree of secularization reached in Athenian society at the close of the Peloponnesian conflict could be ascertained and a tentative answer derived from it. A hypothesis could then be formulated to the effect that further secularization is a function of increased conflict among the value-systems operative within the confines of Athens as a principal secular society tending in the pronormless direction. The fresh evidence, drawn from the period following the Peloponnesian conflict, would then be examined to determine whether or not both secularization and conflict among the designated value-systems actually increased. If the predicted concomitant variation came about within distinctly better than chance limits, the hypothesis could be viewed as validated to that "better than chance" extent; if not, the hunch and its related hypothesis could at best be assigned "negative utility," and at worst should be entirely rejected. In the latter case, a new hunch might then be generated, and a new

hypothesis devised, perhaps necessitating new idiographic and nomothetic types.

A related kind of validation may be called "start and finish." Taking a society at two points in time, where it is thought that prevailing normative patterns are in marked contrast, a question is asked as to how the contrast arose. The society is then compared by means of culture case study at the points of "start" and "finish," and it is found that the initial assumption of contrast between what for some purposes may be viewed as *two* societies seems to be borne out. A tentative answer to the initial question then might read: "In order for Athens of the period immediately after Marathon ('start') to stand in the degree of contrast that it does to the Athens of the period immediately before Chaeronea ('finish'), conflict within the confines of Athens itself between radically discrepant value-systems had probably become evident in marked degree during the intervening years, with extreme secularization as a concomitant." A suitable hypothesis shifting this idiographic hunch into the nomothetic range through the use of type stages, preferably already validated in other contexts, could then be formulated. To validate it, fresh evidence derived from the intervening period between "start" and "finish" should then be examined and the various stages in the secularizing process shown to have varied concomitantly with the intensity of the value-system conflict. It might be useful to summarize the correspondence between the ascertained idiographic succession of events⁶³ and the predicted nomothetic sequence in tabular or similar form, an example of which is presented. (See page 503.)

This tabular presentation, in both form and content, is at best illustrative. The re-

search necessary for setting up either column or a firm footing, and especially for placing them on the same cross-referential level, has been only partially carried out. Nevertheless, "start-finish" validation, although only a variant of the two procedures discussed above, seems to have merit.

In "contemporary" rather than "historical" research, any or all of the three procedures (and several others not discussed here because of lack of space) could be used. Studies of juvenile delinquency, for example, might follow "start-finish" procedure. If a marked contrast between earlier and later American value-systems and degrees of delinquent deviation could be empirically established (perhaps no easy task in view of current arguments as to whether delinquency has actually increased), a hunch might be hazarded that could then be given nomothetic form as a hypothesis incorporating the appropriate "if" and "then" together with the relevant types and their sequential order. Research aiming to establish the idiographic succession of events in American society of 1920, say, to that of 1958,⁶⁴ with special reference to juvenile delinquency, could then be undertaken and the hypothesis proved or disproved.

In "historical sociology" itself there are many ways other than those indicated in which validation of hypotheses can be carried out, or at least attempted in good faith. Common to all of them is one essential point: *no culture case study as such can ever pass beyond plausibility*. Such studies are indispensable if tentative answers likely to be worth the trouble of following up are to be achieved. Nevertheless, hunches, however careful the explorations that generate them, are not hypotheses. And every hypothesis points beyond itself: there is still the problem of validation.

12. CHESS IS NOT POKER

"Logical" is one thing, "psychological" another. And here the temptation to anticipate must be resisted." In the concluding section this earlier passage becomes directly relevant.

⁶³ As given in the table, this idiographic succession of events may seem to be composed merely of a series of battles, wars, alliances, and so on. Nothing could be farther from the writer's intent than to give this impression. The battles, etc., serve simply as convenient pegs on which to hang, did space in the table permit, the specific value-systems involved at approximately the dates noted, the specific discrepancies between them, and the ways in which, by combination and recombination, these discrepancies eventually brought about a specific value-system that failed to sustain the kind of social structure necessary for city-state survival. Please note, then, that tabular pegs for dates are not the same as the significant events that should be hung on them.

⁶⁴ These dates are merely illustrative and quite arbitrary. Properly, "start" and "finish" periods should be polar in some significant way or degree.

AN ILLUSTRATIVE TABLE

	Idiographic	Nomothetic	
"Start"	Athens shortly after Marathon (<i>circa</i> 480 B.C.)	Loyally sacred society becoming vicinally, socially, and mentally accessible	"Start"
"Fresh" Evidence in Idiographic Series	Delian League, 477 B.C. First Peloponnesian War, 460-455 B.C. Second Peloponnesian War, 431-404 B.C. Rule of the Thirty, 404-403 B.C. Continued expansion of Athenian commerce even after defeat, <i>circa</i> 400 B.C. <i>et seq.</i> Rise of Thebes and Macedon as new power centers, 371 <i>et seq.</i> B.C., and inability of Athens to unite in prevention	Incipient loyalistic decline Onset of divided loyalties Internal cleavage in value-system Normative reaction to normlessness Acquiescence in persisting cleavage Loss of principal characteristics and marked increase in consequent, comfortable, and thrilling secularization	"Retrospectively Predicted" Nomothetic Sequence
"Finish"	Athenian dissension and traitorous conduct leading to defeat at Chaeronea, 338 B.C.	Notable decline of varieties of sacredness essential for social control, i.e., extreme secularization	"Finish"

The kind of prediction to which reference has continually been made is retrospective, an aspect of what is often termed *ex post facto* research design. The end-point of the prediction is known; metaphorically, the student who is given the conditions of a mathematical problem is simultaneously provided with the correct answer. But an answer is not a solution. The latter necessitates formulation of the problem on the basis of the given conditions, use of the proper procedures, step by step, and *then* the answer. There are several instances in the history of mathematics, however, where answers were reached without awareness of the processes by means of which they were attained. Long intervals sometimes elapsed before the answers, pragmatically evidenced as correct, could be furnished with appropriate solutions.⁶⁵

⁶⁵ What has been called Case I of "Fermat's Last Theorem" is an apt illustration of this. It is apparently quite correct, pragmatically speaking, but no satisfactory mathematical proof has yet been found.

Henri Poincaré's essay, translated by G. B. Halsted as "Mathematical Creation," in *The Foundations of Science*, New York and Garrison, New York: Science Press, 1913, pp. 383-394, abounds in other instances.

In "historical sociology," clearly enough, no such precision as prevails in mathematics can be achieved. The conditions are strikingly different, but the analogy is nevertheless instructive, if only by contrast. Answers of a sort are usually available before detailed research begins; the fact that the researcher's interest led him to deal intensively with particular historical events or periods, or that his specific interest was aroused by general familiarity with certain historical evidence, means in most cases that he was on the way toward resolving his perplexity. That is to say, partial knowledge of his materials, his problem, his tentative answer, possible ways of justifying his answer, and even his hypothesis, some of the evidence, and the steps necessary for validation were all simultaneously present in mind.

Moreover, retrospective prediction perhaps always involves a tendency to engage in wishful thinking, to "cook" the evidence, and in several other ways to delude oneself and one's fellow-researchers. The inclination to delude is not peculiar to those engaged in retrospective prediction, let us hope, but it certainly carries no checks comparable to the laboratory trial. Successful prospective prediction, pragmatically speaking, is by

far the best start toward convincing validation.⁶⁶

Even in the natural sciences, of course, there are many instances where the right results were secured for the wrong reasons; the success of prospective prediction may not be the result of a definite hypothesis and the marshalling of relevant evidence, but merely the outcome of a lucky hunch. To elevate such an answer to the rank of an acceptable solution, all steps intervening between vague initial perplexity and what finally happened must be filled in and given suitable formulation. Then only is it possible to speak of validation in the full scientific sense.

Whatever the *psychological* handicaps, the *logical* procedure of culture case study, which in some respects is also that of constructive typology, makes it possible for the researcher to detail all of the essential steps. These may be summarized:

1. State the problem in the form of a question, and indicate the whole or wholes thereby defined.
2. Specify the assumptions incorporated in the question, together with the warrant for their use.
3. Focusing on presumably significant normative variations, explore the relevant definitions of situations provided by the subjects concerned, using primary source materials whenever possible.
4. Define the situations involved, explicitly stating when and how such definitions go beyond those offered, in whatever ways, by

⁶⁶ TVTSI, pp. 285-290. But be it noted that we are confronted by many problems of which we dare not await the prospective outcome.

the subjects—that is, show just how the observer's interpretations differ, if at all, from those that the subjects effectively provide.

5. On the basis of the observer's interpretations, give a tentative idiographic answer to the initial question.
6. Formulate a hypothesis, based on the tentative answer, of appropriately nomothetic character.
7. Specify the assumptions embodied in the hypothesis, particularly with regard to the type constructs used, and show the attested nomothetic warrant, at least, for these assumptions.
8. Proceed to validate the hypothesis by the use of suitable evidence, following standards of conditional and probabilistic prediction.
9. Indicate wherever possible the degree to which the hypothesis has been validated.
10. If the hypothesis has been borne out only in part (and this will be the case in virtually all instances), indicate what modifications of it, or what subsidiary hypotheses, should be offered in order to render it of "negative" as well as "positive" utility.⁶⁷

Perhaps all this was succinctly phrased by Habakkuk (ii, 2), "Write the vision, and make it plain upon tables, that he may run that readeth it."

⁶⁷ The steps indicated are suggested only for sociologists trying to make use of historical evidence in ways such that *sociological* standards are met. There is no slightest intent of specifying procedures or objectives for the historian, even when the latter concerns himself with problems of comparison.

In conclusion, it should be noted that aid in the revision of this article has come from sociologists and from historians. Among the former should be listed Thomas D. Eliot, Burton K. Fisher, John Rhoads, and Norman B. Ryder; among the latter, Christopher Bennett Becker and August Meier.

CONVENTIONAL VERSUS METROPOLITAN DATA IN THE INTERNATIONAL STUDY OF URBANIZATION

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MOST of urban sociology rests upon observations made in countries representing a small and biased sample of the world as a whole. Any attempt, how-

ever, to remove this narrow restraint—that is, any attempt to extend comparative urban analysis to include all parts of the earth—runs into the complex problem of comparability. In the present paper we have no solution to offer for this problem as it affects all aspects of urban sociology, but we

* Expanded version of a paper read at the annual meeting of the American Sociological Society, Seattle, Washington, August, 1958.

do have some information bearing upon the validity of one type of international comparison—namely, the degree of urbanization as between one country and another. This is perhaps the type of urban statistic most frequently used in comparing countries. The proportion of people living in urban places or in cities of a given size is considered a fundamental trait of any society. The data required for computing such proportions are, for many countries at least, readily obtainable and hence widely used.

In view of the importance and wide use of data on the degree of urbanization in countries and regions, it is essential that such data be at least approximately comparable from one area to another. There must be assurance that persons included as city residents in one country are not excluded in other countries, and vice versa. The researcher who makes comparisons of the extent of urbanization must normally, however, rely on the statistics that governmental agencies provide on cities. He therefore has control neither over the type of data reported nor over the demarcation of towns and cities as statistical units. Since, as is well known, different governments follow different procedures in delimiting their urban units, there is great danger that apparent differences among countries in the proportion of the population living in towns or cities may be in part the result of contrasting definitions and statistical practices. Even within the same country there may be little uniformity from one province or state to another or from one city to another in drawing the urban boundaries.

The lack of comparability stems, of course, from the fact that the so-called "city" is often a political or administrative unit. As such, its boundaries may or may not approximate the actual limits of the demographic or ecological "city." Some cities, as defined by the government concerned, are "underbounded" in the sense that their territory embraces only a part of the total area that makes up the ecological city and their population includes only a part of the total urban aggregate; others are "overbounded" in the sense that their limits extend far beyond the city as a

continuous urban area, embracing land that is rural by virtually any standard.¹

This situation obviously poses a dilemma for the student of urban phenomena. On the one hand, he must of necessity often work with data on administratively defined cities; yet on the other hand he knows that the data are not strictly comparable. One way to resolve this dilemma would be to provide the student of international urbanization with an idea of the actual degree of comparability or incomparability of official data on urban proportions in the world's nations. This could best be done, of course, if by some magic we had information on the populations of the actual demographic cities of the earth, for then we could compare the official data with the correct data. Since we do not generally have this information, the next best procedure is to compare the official statistics with the data on metropolitan areas delimited throughout the world according to a roughly comparable standard. Such a task is now possible because the International Urban Research office has just finished a world-wide delimitation of Metropolitan Areas of 100,000 or more inhabitants. The present paper therefore attempts to assess the validity of international comparisons of degree of urbanization based on official statistics by checking such comparisons against those made with our data on Metropolitan Areas. The question is not only whether or not *any* comparison of national urbanization based on official statistics is valid, but also which kinds of official statistics on urbanization are *most* valid.

TREATMENT OF DATA AND SOURCES

Since the procedures employed in our world-wide delimitation of Metropolitan

¹ In most industrial countries, the "cities" are underbounded. In the United States in 1950, for example, the central cities held only 70 percent of the residents and only 49 percent of the land encompassed in the Urbanized Areas. In the case of the Philippines, on the other hand, some of the "Chartered Cities" are overbounded to an extreme degree, for they embrace great stretches of purely rural and sometimes uninhabited land. A fuller treatment of the question of urban delimitation will be found in International Urban Research, *The World's Metropolitan Areas*, to be published in 1959 by the University of California Press.

Areas are set forth at length in another publication,² we shall describe them only briefly. We began with a list of all administratively defined cities or continuous urban areas of 50,000 or more, designating them as principal cities. Then the administrative or territorial units around these cities were examined to determine the percentage of their labor force engaged in agriculture and their distance from the principal city. To be included in a Metropolitan Area, an administrative unit had to (1) touch upon the principal city or an administrative area already included in the M.A.; (2) have at least 65 per cent of its labor force engaged in non-agricultural occupations; (3) be located close enough to the principal city to make commuting feasible. In some cases the

lack of data made it necessary to substitute a density criterion for the non-agricultural criterion, in which case any unit included in the M.A. had to have a density either equal to half or more of the density of the principal city (or the next inner ring) or twice that of the next outer ring. If the boundary of the area established by these criteria failed to include at least 100,000 people, we dropped it from the list.

This procedure for delimiting metropolitan areas, crude as it may be, is sufficiently standardized to guarantee a high degree of comparability. Furthermore, it can be applied to most of the world's countries and particularly to those containing numerous cities. It therefore furnishes a yardstick for assessing the validity of comparisons made on the basis of officially reported urban statistics.

² International Urban Research, *op. cit.*

TABLE 1. PER CENT OF TOTAL POPULATION URBAN AND PER CENT IN METROPOLITAN AREAS AND LOCALITIES OF SIX SIZE RANGES BY COUNTRIES, Circa 1950

Countries by Type of Locality *	Per Cent of Total Population in Localities by Size Ranges **						Per Cent Urban **	Per Cent in Metropolitan Areas ***
	2,000 +	5,000 +	10,000 +	20,000 +	50,000 +	100,000 +		
Type A								
Argentina, 1947	62.5	56.9	52.7	48.3	42.1	37.2	62.5	43.8
Australia, 1947	74.2 ¹	66.4 ¹	61.6 ¹	57.3 ¹	52.4 ¹	51.4 ¹	68.9	55.4
Cuba, 1953	50.9 ¹	45.0 ¹	40.9 ¹	36.5 ¹	28.5 ¹	21.9 ¹	57.0 ¹	26.0
Denmark, 1950 ^a	58.8	55.7	51.4	44.8	36.7	33.5	67.3	45.5
France, 1954	50.2 ¹	42.4 ¹	36.8 ¹	29.8 ¹	21.2 ¹	15.0 ¹	55.9	34.4
India, 1951	37.7	21.1	15.3	12.0	8.7	6.6	17.3	7.8
Ireland, 1951	40.5	35.5	32.2	28.3	23.7	17.6	41.5	23.4
Israel, 1949 ^b	73.6 ¹	66.2 ¹	61.9 ¹	51.3 ¹	45.6 ¹	45.6 ¹	71.3 ¹	55.9 ¹
Italy, 1951	56.5 ¹	45.4 ¹	37.5 ¹	30.3 ¹	21.9 ¹	17.0 ¹	40.9 ¹	27.3
Netherlands, 1947	72.6	63.6	56.1	49.8	41.3	32.7	54.6	45.6
Norway, 1950	44.1	40.3	38.4	32.7	26.0	19.8	32.2	21.8
Pakistan, 1951	10.2 ¹	10.0 ¹	9.1 ¹	8.0 ¹	5.9 ¹	5.1 ¹	11.4	5.1
Portugal, 1950	31.2	24.6	19.4	16.4	12.7	12.7	31.2	19.6
Sweden, 1950	51.9	45.4	40.3	33.0	25.5	19.4	47.5	31.8
United States, 1950 ^d	65.1 ¹	60.3 ¹	56.2 ²	52.0 ²	46.8 ¹	43.9 ¹	64.0	55.9
Type B								
Australia, 1947	71.5 ¹	62.9 ¹	55.4 ¹	43.9 ¹	17.2 ¹	3.0 ¹	68.9	55.4
Brazil, 1950	30.8	26.8	23.4	20.2	16.3	13.2	36.2	17.6
Canada, 1951	50.7	45.3	40.2	35.1	27.5	23.3	61.6	45.5
Ceylon, 1946	15.2	14.8	14.0	11.4	8.7	5.4	15.4	9.5
Colombia, 1951	34.7 ¹	29.1 ¹	25.5 ¹	22.4 ¹	18.2 ¹	14.7 ¹	36.3	18.6
Costa Rica, 1950	28.6	23.7	18.8	10.9	10.9	0.0	33.5	19.9
Dominican Republic, 1950	21.5	18.5	16.0	11.1	11.1	8.5	23.8	11.2
Ecuador, 1950	27.7	24.0	21.3	17.8	14.6	14.6	28.5	14.9
El Salvador, 1950	27.6	21.7	17.3	12.9	11.5	8.7	36.5	11.9
Finland, 1950	35.1	31.6	28.2	22.2	14.2	14.2	32.3	17.0
Greece, 1951	49.7 ¹	38.3 ¹	33.9 ¹	26.8 ¹	16.0 ¹	12.7 ¹	36.8	22.0
Guatemala, 1950	23.9	16.8	12.5	11.2	10.2	10.2	25.0	10.6
Haiti, 1950	10.0	8.2	6.3	5.1	4.3	4.3	12.2	6.0
Honduras, 1950	17.2	11.8	9.8	6.8	5.3	0.0	31.0	7.3

TABLE 1.—(Continued)

Countries by Type of Locality *	Per Cent of Total Population in Localities by Size Ranges **							Per Cent Urban ***	Per Cent in Metropolitan Areas ***
	2,000 +	5,000 +	10,000 +	20,000 +	50,000 +	100,000 +			
India, 1951	17.3 ¹	16.7 ¹	14.4 ¹	11.9 ¹	9.0 ¹	7.2 ¹	17.3	7.8	
Japan, 1950	59.6 ¹	58.0 ¹	50.7 ¹	42.1 ¹	33.2 ¹	25.6 ¹	37.5	36.3	
Malaya, 1947	24.3	21.2	19.0	17.1	10.2	7.4	26.5	12.7	
Mexico, 1950	45.5 ¹	34.6 ¹	28.9 ¹	24.0 ¹	18.7 ¹	15.1 ¹	42.6 ¹	20.3	
New Zealand, 1951	65.7	60.2	57.0	54.2	41.6	32.8	61.3	43.6	
Nicaragua, 1950	28.0	21.7	19.0	15.2	10.3	10.3	34.9	13.3	
Panama, 1950	42.5	33.8	27.8	22.4	22.4	15.9	36.0	23.9	
Paraguay, 1950	28.1	20.2	18.4	15.2	15.2	15.2	34.6	15.6	
Peru, 1940	25.5 ¹	20.4 ¹	17.5 ¹	13.9 ¹	10.5 ¹	8.4 ¹	36.1 ¹	10.4	
Philippines, 1948	21.0 ¹	13.8 ¹	8.9 ¹	6.3 ¹	4.1 ¹	3.4 ¹	24.1 ¹	10.3	
Thailand, 1947	9.9 ¹	9.8 ¹	8.9 ¹	6.7 ¹	4.5 ¹	4.5 ¹	9.9	6.8	
Turkey, 1950	28.7	22.4	18.7	14.5	10.1	8.2	21.9	14.0	
Union of South Africa, 1951	39.8	36.2	33.2	30.7	28.2	24.0	42.6	31.5	
United Kingdom, 1951	79.7 ¹	77.6 ¹	74.0 ¹	66.9 ¹	50.8 ¹	36.1 ¹	80.3 ¹	77.0	
United States, 1950	59.8 ¹	54.4 ¹	49.0 ¹	43.0 ¹	35.3 ¹	29.4 ¹	64.0 ¹	55.9	
Venezuela, 1950	49.7 ¹	42.4 ¹	36.8 ¹	32.2 ¹	24.7 ¹	20.6 ¹	53.8	26.2	
Type C									
Austria, 1951	65.7	49.3	43.1	39.8	35.2	32.9	49.2	38.9	
Belgium, 1947	82.3	62.7	46.6	32.0	17.9	10.5	62.7	41.4	
Egypt, 1947	90.9	64.1	40.3	29.1	22.7	19.3	30.1	19.6	
France, 1954	62.6	49.8	41.5	33.3	23.1	16.8	55.9	34.4	
Germany, East, 1950 ^c	70.1 ¹	57.2 ¹	47.7 ¹	39.2 ¹	25.6 ¹	20.8 ¹	67.6 ¹	37.9	
Germany, West, 1950 ^c	72.5	59.5	50.9	44.1	35.7	30.3	72.4 ¹	51.2	
Greece, 1951	55.4	42.0	36.3	28.1	16.2	12.7	36.8	22.0	
Iraq, 1947	99.9	99.5	94.7	76.9	34.9	16.6	33.8	17.5	
Italy, 1951	93.1	73.9	55.4	41.2	28.0	20.4	40.9 ¹	27.3	
Japan, 1950	98.0	75.1	53.9	42.4	33.2	25.6	37.5	36.3	
Philippines, 1948	99.4	97.2	85.7	55.5	17.5	9.3	24.1	10.3	
Spain, 1950 ^b	83.2	66.3	51.8	39.8	30.3	24.1	37.0	24.2	
Switzerland, 1950	68.1	48.1	36.4	29.1	24.7	20.6	36.5	41.2	
Thailand, 1947	100.0	99.9	99.4	95.2	57.4	13.3	9.9	6.8	

* See text for a description of the three types of localities.

** Unless designated otherwise the percentages shown in these columns are based on figures reported in the *Demographic Yearbook, 1955*, Tables 7 and 8.

*** Based on provisional figures prepared by International Urban Research.

^a Excluding the Faeroe Islands; ^b Per cent urban for the year 1952; ^c 1951; ^d Urbanized Areas and incorporated or unincorporated places outside of Urbanized Areas treated as localities; ^e Estimate made by International Urban Research; ^f Including East Berlin; ^g Including West Berlin; ^h Including Canary Islands; ⁱ Census reports or official yearbooks used as source.

The present analysis is made with reference to the 50 independent countries shown in Table 1, representing approximately half of the world's population and 42 per cent of its inhabited land area. For each country, as explained above, we utilize two kinds of data: our M.A. delimitations derived from the last census prior to 1954;³ and the officially reported data on localities. Localities

for which populations are officially reported fall into different classes according to how the areas are delimited by the government concerned; and this fact in part accounts for the scepticism of many people concerning the comparability of urban data from one country to another. The United Nations, in its latest report on the population of localities by countries,⁴ distinguishes three classes, as follows: Type A—agglomerations or clusters of population without regard to official

³ This dating was violated in two cases: France, where the census was taken *in* rather than *prior* to 1954, and Ceylon, where the census of 1946 was used instead of that of 1953 (which was un-available).

⁴ United Nations, *Demographic Yearbook, 1955*, New York: 1955, Table 8, pp. 198-215.

boundaries or administrative functions; Type B—localities having fixed boundaries and an administratively recognized "town" status, usually characterized by some form of local government operating under a charter or terms of incorporation, and normally called by some such term as "city," "borough," "urban district," or "municipality"; Type C—minor civil divisions (often the smallest of the administrative divisions) which have fixed boundaries and which together comprise the entire territory of the country.⁵ Our analysis in this paper undertakes to deal with each of these classes separately. For officially reported populations of localities in each class we have drawn upon the valuable data provided by the United Nations Statistical Office in the *Demographic Yearbook* (cited above), where the localities are grouped by ten size-classes. It has proved necessary, however, to go beyond the Statistical Office's data, either by including additional countries or data on one of the types of locality not provided for a particular country.⁶

For each of the 50 countries the reader will find in Table 1 the percentage of the nation's total population residing in localities of six open-ended size-classes. The countries are grouped in this table according to the type of locality used as a reporting unit—Types A, B, and C. Some countries appear twice in the table because for them two types of localities are reported. To be sure, questions concerning the proper classification of countries by type of locality used as a reporting unit present a problem. In several cases it is difficult to ascertain the exact status of the localities, and in other cases it appears that the localities constitute a mixture of Types A and B. The seriousness of this problem is lessened, however, by our results, as will be seen presently.

Having, on the one hand, the percentage of the population living in M.A.s of 100,000

or over based on our own delimitations⁷ and, on the other hand, the officially reported data for percentage "urban" and percentage in six size-classes of locality (grouped into three definition-classes), we proceeded to run correlations as a test of the validity of the officially reported information.

ANALYSIS OF FINDINGS

The results, shown in Table 2, strikingly belie the hypothesis that officially reported statistics on either urban agglomerations (Type A) or administratively defined cities and towns (Type B) are grossly non-comparable as between countries. The product-moment coefficients of correlation (r) between the percentage in any size-class of urban agglomerations or clusters (Type A) and the percentage in M.A.s is .91 or above. As could be expected, the correlations are generally lower between the percentages in cities and towns defined administratively (Type B) and the proportion in M.A.s; but the difference between this and the Type A case is amazingly small.

Special interest attaches to the last column of Table 2. The proportion of the population said to be "urban" presumably varies from one country to the next in part because the dividing line between "rural" and "urban" is arbitrary and unstandardized. The United Nations *Demographic Yearbook* for 1955 (p. 16) lists several reasons for noncomparability in this respect, and Table 7 in this issue, showing the proportion urban by countries, is careful to include in each case the definition of "urban."⁸ As the last column of our Table 2 shows, however, the defects in the official distinction between "rural" and "urban," although real and admittedly undesirable, do not render meaningless the reported figures on the percentage urban. In fact, so long as the countries are

⁷ The M.A. percentages given in Table 1 must be considered provisional.

⁸ The *Demographic Yearbook* for 1952 followed the same procedure in giving data by countries for the proportion urban. It also included a discussion of the validity of the data, concluding (on pp. 9-10) that international comparisons of the percentage urban should be avoided "except in the most general way." However, it was found (p. 11) that the percentage urban correlates with the percentage in cities of 100,000 or more to the extent of 0.84.

⁵ *Ibid.*, p. 17.

⁶ In the case of France, for example, the localities reported by the United Nations are minor civil divisions (Type C). We have made use of these figures in our analysis, but have also obtained data from French census reports on agglomerations (Type A).

INTERNATIONAL STUDY OF URBANIZATION

509

TABLE 2. COEFFICIENTS OF CORRELATION (r) BETWEEN PER CENT IN METROPOLITAN AREAS AND SEVEN INDICES OF URBANIZATION BY COUNTRIES, Circa 1950

Countries by Type of Locality	Per Cent in Localities by Size Range						Per Cent Urban *
	2,000+	5,000+	10,000+	20,000+	50,000+	100,000+	
15 Countries with Type A Localities (urban aggregates or clusters)	+ .91	+ .96	+ .96	+ .96	+ .96	+ .95	+ .94
30 Countries with Type B Localities (administratively demarcated "towns" and "cities")	+ .92 *	+ .94 *	+ .95 *	+ .95 *	+ .95 *	+ .90 *	+ .92
14 Countries with Type C Localities (minor civil divisions)	- .56	- .68	- .68	- .59	- .19	+ .59	+ .86
Countries with Types A or B Localities	+ .92 *	+ .95 *	+ .96 *	+ .95 *	+ .95 *	+ .91 *	+ .92 *

* Per cent urban as administratively defined. These figures have no necessary connection with the types of localities as defined in the stub, but simply characterize the countries in question.

* Excluding Australia, N=29. If Australia is included as a Type B country (N=30), the correlations for each size-class, from left to right, are as follows: .93, .95, .95, .95, .89, and .74.

* Excluding Australia as a Type B country, N=44. With Australia included as a Type B country (N=45) the corresponding correlations, from left to right, are: .93, .95, .96, .95, .91, and .82.

* Excluding all Type B countries which are also included as a Type A, N=42.

using definitions that relate to Type A or Type B localities, regardless of the exact rural-urban dividing line they prescribe, the proportion urban constitutes an index of national urbanization that can be used with some degree of confidence.

Because the boundaries of Type A and Type B localities are drawn on a different basis, one might infer that an index of urbanization based on the one is not comparable with an index based on the other. Although this is true in the sense that there is an obvious danger in mixing the two, especially if individual countries are being compared, the fact remains that both classes are highly correlated with the proportion in M.A.s. Furthermore, when the two groups are combined, as in the last line of Table 2, the correspondence with the proportion in 100,000-plus M.A.s remains very high. We can thus conclude that in comparing groups of countries with reference to urbanization, those that define localities in Type A terms can be compared to those that define localities in Type B terms with relative safety.

Our results, however, are by no means uniformly positive. We shall have occasion

below to sound a warning about the use of official data on urban places *in general*. For the moment, let us cite the results referring specifically to countries that publish data on localities defined in Type C terms. It will be recalled that these are minor civil divisions (usually the smallest in the country) which, taken together, comprise the entire national area. In other words, they are territorial divisions like "counties" in the United States, "taluks" or "thanas" in India, or "municipios" in Latin America. As shown by line 3 in Table 2, the proportion of the population in such localities by size-class varies inversely with the proportion of the population in Metropolitan Areas. The only exception involves the minor civil divisions having a population of 100,000 or more; for when they become this large they apparently are in many cases urban and there is a correlation, albeit not a strong one, with the percentage in M.A.s. It seems clear that the proportion of the population in Type C localities, regardless of size-class, cannot be used with any security for purposes of international comparison.

A GENERAL WARNING

In addition to noting the unacceptability of Type C data for urban comparisons, it must be stressed that no international statistics on towns and cities should be used without critical examination. The point can be illustrated by reference to two cases, Australia and Thailand. Australia follows the extremely unusual practice of cutting up its urban agglomerations into very small politically-defined "cities." Therefore, if the figures for these "cities" (which are Type B units) are utilized uncritically as they stand, one winds up by reporting what is actually one of the world's most urbanized countries as having only 3 per cent of its population living in "cities" of 100,000 or more! When Australia is included in Table 2 as one of the countries in the Type B category, the correlations drop markedly, as shown in the table's footnotes. Fortunately, Australian census reports cover actual aggregates as well as politically defined places, and, as shown in Table 1, we need not depend on the Type B data for an index of urbanization in that country. But it is clear that uncritical use of Australian statistics for political localities might easily produce erroneous conclusions.

The case of Thailand shows the error of the uncritical use of the percentage urban as administratively defined. In this country the communes of more than 2,500 inhabitants are evidently designated in official reports as urban.⁹ Since the commune is a minor civil division, this practice is tantamount to according urban status to all counties in the United States having a population of, say, 10,000 or more.

When the percentage urban in Thailand as reported in the Thai Yearbook (90.85) is used rather than the figure provided by

the U.N.'s Statistical Office, the correlation between the percentage urban and the percentage in M.A.s is +.69 among Type B countries, +.22 for Type C countries, and +.76 when Types A and B countries are combined. The discrepancy between these correlations and the corresponding ones in Table 2 indicates that the percentage urban as administratively defined may in certain isolated cases bear no relationship whatever to the true extent of urbanization in a country. This is particularly the case when, as in Thailand, the urban-rural distinction is based on the population size of minor civil divisions (Type C localities).

THE CUTTING POINT FOR THE RURAL-URBAN DISTINCTION

One reason, of course, why the correlations in Table 2 show so little variation is that the size-classes are open at the top and therefore cumulative. In other words, the size-class 5,000 and over behaves statistically much like the class 10,000 and over because it is made up principally by the latter class. In the United States, for example, the 10,000-plus class of places includes 90.1 per cent of the population of all places of 5,000-plus.

To what extent, then, will the size-classes correlate with the metropolitan percentage when they are defined as mutually exclusive classes? The expectation, of course, is that on the whole the correlations will be reduced—and this is what happens, as Table 3 shows. But the variations are interesting. In Column 1, for example, it appears that the percentage of the population in places of 2,000 to 5,000 bears no relation to the percentage in metropolitan areas of 100,000 or more. The explanation is doubtless the following: The proportion of people who live in places of this size is not a function primarily of economic development or per capita wealth, but a function of the pattern and the density of rural settlement. In a country such as India, for instance, the proportion in places of this size would be comparatively high, not because India is highly urbanized but because the Indian countryside is very densely settled and its rural people follow the custom of congregating in villages rather than living in isolated farmsteads. In the United States a lesser proportion can be

⁹ *Statistical Yearbook of Thailand, 1952*, Bangkok: Central Statistical Office, 1953, Table 15, p. 42. Interestingly, the United Nations, in its *Demographic Yearbook, 1955*, p. 193, does not utilize the figure given in the Thai yearbook, but instead gives a much more realistic figure for the percentage urban—9.9. This is the figure we have used in Table 1. How this percentage was obtained is not known, but in another table, p. 207, the *Demographic Yearbook* places the Thai localities into Type C and gives figures showing 99.9 percent of the population as living in localities of more than 5,000 inhabitants.

INTERNATIONAL STUDY OF URBANIZATION

511

TABLE 3. COEFFICIENTS OF CORRELATION (r) BETWEEN PER CENT IN METROPOLITAN AREAS AND FOUR COMPONENT INDICES OF URBANIZATION BY COUNTRIES, Circa 1950

Countries by Type of Locality	Per Cent in Localities by Size Range of Locality			
	2,000-4,999	5,000-9,999	10,000-19,999	20,000-49,999
Countries with Type A Localities N=15	-.08	+.26	+.55	+.40
Countries with Type B Localities* N=29	+.01	+.43	+.54	+.75
Countries with Types A or B Localities* N=44	+.07	+.43	+.58	+.67

* Australia excluded as a Type B country.

expected to be in such places, because the farmers tend to live on farmsteads and the business people tend to live in places larger than 5,000. Inspection of the actual proportions shows the expectations to hold true, as follows:

POPULATION IN PLACES OF 2,000-4,999

Number	As % of Total Population	As % of Population in All Places 2,000+	
		Population	in All Places 2,000+
India, 1951, Type A	59,108,973	16.6	44.0
U.S.A., 1950, Type A	7,290,205	4.8	7.4

We know that the United States is far more urban than India. The fact that it has a lesser proportion in towns of 2,000 to 5,000 inhabitants has nothing to do with the degree of urbanization in the two countries but a great deal to do with the average density and the residential agglutination in rural areas.

The data of Table 3 suggest that the cutting point between rural and urban should not be assumed, as most governments so do, to be somewhere between 2,000 and 5,000. This may be the appropriate range for a cutting point in some countries but not in others. An arbitrary boundary applied to all countries within this range would thus lead to distortion rather than to comparability. Actually, the best cutting point probably does not lie even in the 5,000 to 10,000 range. If a lower limit to the size

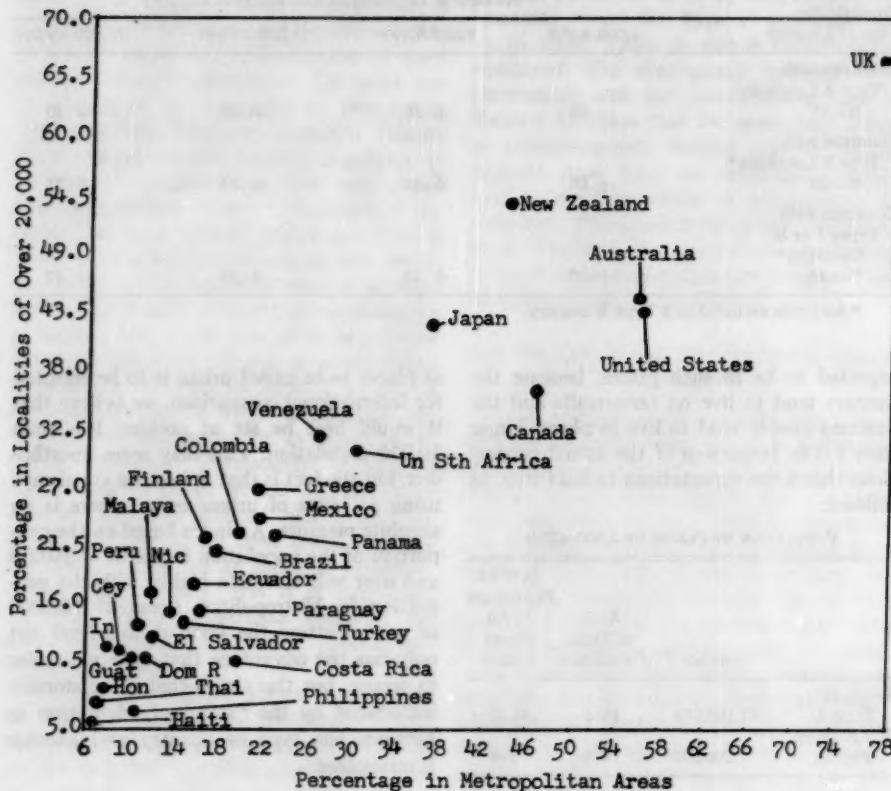
of places to be called urban is to be adopted for international comparison, we believe that it would best be set at nothing less than 10,000 population. This may seem unorthodox, but the fact is that at best we are always using an *index* of urbanization; there is no absolute measure. An index based on the proportion of the population in places of 10,000 and over will correlate highly with the proportion in Metropolitan Areas of 100,000 or over. Putting the floor at this level not only has the advantage that data are easier to secure, but the virtue that the distortion introduced by the "standard" definition as between one type of country and another is minimized.

THE DEVIANT CASES AND URBAN ANALYSIS

In all studies of statistical association, special interest attaches to the deviant instances. In the present case, we may view the marked divergence of any country from the regression line as an invitation to investigate either the validity of the Metropolitan Areas delimited for it or the validity of its reporting for Type A or Type B localities.

In Figure 1, which shows a scatter diagram involving countries that supply data on Type B localities, the United States appears to be quite deviant, having a higher proportion in metropolitan areas than the percentage in cities and towns of 20,000 or more would seem to justify. Since the Standard Metropolitan Areas delimited by the Census Bureau were used for the United States, does this deviation from the trend

FIGURE 1. THE RELATIONSHIP BY COUNTRIES BETWEEN THE PERCENTAGE OF THE POPULATION IN TYPE B LOCALITIES OF OVER 20,000 AND THE PERCENTAGE IN METROPOLITAN AREAS,
Circa 1950



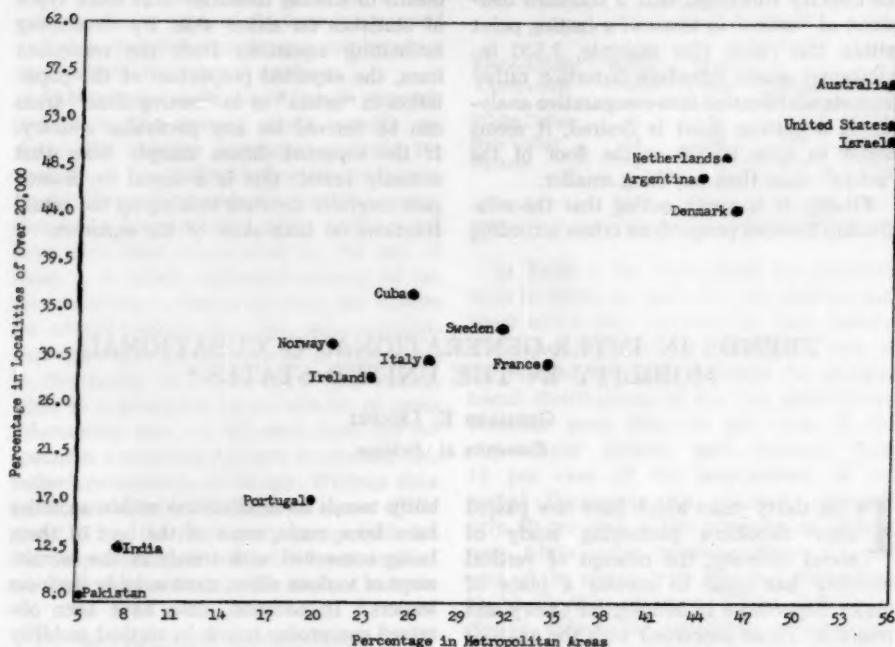
line mean that the S.M.A.s are larger than they should be, as many critics have alleged; or does it mean that localities of over 20,000 are not fully representative of the degree of urbanization because, by the Type B definition, many places that are parts of big urban agglomerations are necessarily omitted? On the basis of Figure 1 alone, we would have no way of settling this issue, but the solution is suggested by Figure 2. There it is seen that when Type A units are used—that is, actual urban aggregations, in this case Urbanized Areas delimited by the Census Bureau—the proportion in M.A.s in the United States is much nearer the line of best fit. In other words, it looks as if the deviation of the United States in Figure 1 is the fault of the "cities of 20,000

and over" rather than the fault of the "Standard Metropolitan Areas."

A situation similar to that in the United States is found in the case of Australia. Whereas it deviates from the trend line in Figure 1, it is very close to the line in Figure 2. Thus, as suggested earlier, the Type A localities provide the most valid gauge of urbanization in Australia.

We cannot pursue this line of analysis in the present paper, but Figure 1 suggests that Canada, the United States, Australia, and the United Kingdom all fail more than most countries to include the whole urban population in their administrative "cities" and "towns." In such countries there is particular reason to develop data of both the type called "metropolitan areas" and

FIGURE 2. THE RELATIONSHIP BY COUNTRIES BETWEEN THE PERCENTAGE OF THE POPULATION IN TYPE A LOCALITIES OF OVER 20,000 AND THE PERCENTAGE IN METROPOLITAN AREAS,
Circa 1950



the type called "urbanized areas." Further analysis of this situation would greatly contribute to a clarification of the weaknesses and strengths of existing types of urban statistics.

CONCLUSION

Since official statistics on urbanization have been and will continue to be used for international comparisons, their validity should be assessed as carefully as possible. Although no absolute yardstick exists by which the accuracy of the official data can be judged, we have utilized as a means of assessment an index of urbanization that is more standardized than any other available—namely, the proportion in Metropolitan Areas of 100,000 or more inhabitants, areas we delimited according to common criteria. The correlations between this index and indices of urbanization based on official statistics run so high as to justify the conclusion that the latter, when carefully selected and critically used, possess a high

degree of validity for comparative research purposes. Since the individual researcher lacks the resources necessary to construct measures of urbanization that go beyond official statistics, this conclusion should encourage scholars to make full though careful use of officially published urban statistics for comparative research.

Of course, the results of our analysis do not prove that the boundaries of all cities are comparable. The findings indicate only that selected current official statistics on urban aggregates or administratively defined cities and towns (Type A and B localities) contain no bias so serious as to yield an unrealistic index of urbanization. The question of the comparability of the official boundaries of individual cities is a matter not dealt with here.

A further consequence of our investigation is the conclusion that the size-classes of places from 2,000 to 10,000 are of little use in constructing an index of the degree of urbanization. The frequency of towns

of this range in a population is so much a matter of the particular circumstances of the country concerned that a standard definition of "urban" in terms of a cutting point within this range (for example, 2,500 inhabitants) would introduce distortion rather than standardization into comparative analysis. If a cutting point is desired, it seems better to take 10,000 as the floor of the "urban" class than anything smaller.

Finally, it is worth noting that the relationship between proportions urban according

to reported urban statistics and the proportions urban according to M.A. data offers means of finding countries with weak types of statistics on either side. By developing estimating equations from the regression lines, the expected proportion of the population in "urban" or in "metropolitan" areas can be derived for any particular country. If the expected differs sharply from that actually found, this is a signal to investigate carefully the data making up the actual fractions on both sides of the equation.

TRENDS IN INTER-GENERATIONAL OCCUPATIONAL MOBILITY IN THE UNITED STATES *

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In the thirty years which have now passed since Sorokin's pioneering study of social mobility, the concept of vertical mobility has come to assume a place of major importance in sociological theory and research. Those concerned with the analysis of societal dynamics have increasingly stressed the importance of changes in either the nature or volume of vertical mobility. Unfortunately, however, it has not been an easy task to deal empirically with such changes in whole societies, owing to the spatial and temporal dimensions of the phenomena involved. Thus, today we still lack the data required to answer with any assurance the question of whether the rate of vertical movement in our own or other societies has increased, decreased, or remained constant. Without such data, discussions of the causes and consequences of shifts in the rate of vertical movement are necessarily speculative.

To be sure, progress has been made in the investigation of societal trends. Since the 1920s several excellent studies of mo-

bility trends in subdivisions within societies have been made, some of the best of them being concerned with trends in the recruitment of various elites, most notably business leaders.¹ In addition, data have been obtained concerning trends in vertical mobility in certain localities.² But only two studies have dealt with the problem of *trends* at the societal level: Glass's investigation of inter-generational occupational mobility in Britain; and the volume by Jaffe and Carleton on *intra-generational* occupational mobility in the United States.³

The research reported here involves an age cohort analysis of inter-generational mobility in a nation-wide sample of adult males. The data were obtained by the Survey Research Center of the University of Michigan in its study of the 1952 presidential election. The basic characteristics of the sample together with the findings of that enquiry are

¹ Some of the more important of these studies conducted in the United States have been summarized in Bernard Barber, *Social Stratification*, New York: Harcourt, Brace and Co., 1957, pp. 443-467.

² The best of these studies is Natalie Rogoff, *Recent Trends in Occupational Mobility*, Glencoe, Ill.: Free Press, 1953.

³ D. V. Glass (editor), *Social Mobility in Britain*, London: Routledge & Kegan Paul, 1954, Chapter 8; and A. J. Jaffe and R. O. Carleton, *Occupational Mobility in the United States 1930-1960*, New York: King's Crown Press, 1954.

* The writer wishes to express his grateful appreciation to Angus Campbell for making available the data from his 1952 presidential election study for use in this analysis, and to Morris Janowitz, Werner Landecker, and G. E. Swanson for critically reading the original draft of the manuscript and making valuable suggestions for its improvement.

described by Campbell, Gurin, and Miller in *The Voter Decides*.⁴ It should be noted that 1,799 adults selected from the American population as a whole were interviewed and this clustered sample was employed as the basis of the present analysis.

Although the exclusion of female respondents from the present study greatly reduced the number of cases available for analysis, it was believed that this reduction would eliminate sources of possible confusion in the interpretation of the findings and thus more than compensate for the loss of cases. A small additional number of respondents were eliminated from the sample for several reasons. In a few cases respondents reported the absence of a male head in the family in which they were raised, while in a somewhat larger number of cases information was not obtained from the respondent concerning his own occupation, his father's occupation, or his age. Without data on these variables such cases were eliminated. Thus the final study group included 747 respondents, or almost 95 per cent of the male respondents in the original sample.

METHODOLOGY AND FINDINGS

For purposes of the present analysis, respondents and their fathers were divided into three occupational categories: white collar workers, blue collar workers, and farmers. A more detailed breakdown would have been preferable, but in view of the limited number of cases and the fact that in the original study by Campbell and his associates skilled workers and semi-skilled workers were coded alike, it seemed wiser to restrict the analysis to these three basic categories.⁵

⁴ A. Campbell, G. Gurin, and W. Miller, *The Voter Decides*, Evanston, Ill.: Row, Peterson and Co., 1954.

⁵ Needless to say, definitions of occupational categories are in some degree arbitrary. The three selected here, however, constitute perhaps the basic minimum for any study of mobility in a national sample. Clearly, the number of categories employed could be greatly increased by taking account of the variations which exist within each of these basic categories. Using a greater number of categories would almost surely result in higher rates of mobility, but the greater the number of categories employed the less significant the movement would be. In view of the limited knowledge in the

TABLE 1. OCCUPATIONS OF MALE RESPONDENTS BY OCCUPATIONS OF RESPONDENTS' FATHERS

Respondent's Occupation	Respondent's Father's Occupation:			Total
	White Collar	Blue Collar	Farmer	
White Collar	99	87	69	255
Blue Collar	52	188	139	379
Farmer	2	5	106	113
Total	153	280	314	747

In Table 1 the respondents are classified both in terms of their own occupations and those which they reported for their fathers. The most notable fact indicated here is the marked difference between the occupational distributions of the two generations. Whereas more than 40 per cent of the respondents' fathers were farmers, only 15 per cent of the respondents so reported themselves. In contrast, although only 20 per cent of the respondents' fathers were white collar workers, almost 35 per cent of the respondents reported themselves to be in this category. These differences clearly reflect the changing occupational structure of American society. The fact that nearly half of the respondents assign themselves to an occupational category other than that of their fathers suggests a tremendous volume of mobility during the period in question.

One must raise the question, however, of whether all of this movement between occupational categories can legitimately be regarded as *vertical* mobility. On the basis of Table 1, a rather strong case can be made for the view that the movement between the blue collar and farming occupations involves no significant change in the social status of the individuals concerned. The life-chances of farmers' sons in the urban job market seem to be almost identical with those of blue collar workers' sons: in both cases only about one-third of the sons achieved white collar status. If, on the basis of such evidence, inter-generational movement between these two occupational categories is not defined as vertical mobility, the proportion of persons in the sample classified

field, it seemed appropriate to concentrate first on the more basic categories.

TABLE 2. OCCUPATIONS OF MALE RESPONDENTS BY
OCCUPATIONS OF RESPONDENTS' FATHERS, BY
DATE OF BIRTH OF RESPONDENTS, IN
PERCENTAGES

Date of Birth of Respondent	Respondent's Father White Collar Worker				N
	White Collar	Blue Collar	Farmer	Total	
1923-1932	60.0	40.0	...	100.0	20
1913-1922	66.7	33.3	...	100.0	42
1903-1912	52.3	45.5	2.3	100.1	44
1893-1902	79.2	16.7	4.2	100.1	24
1853-1892	73.9	26.1	...	100.0	23

Date of Birth of Respondent	Respondent's Father Blue Collar Worker				N
	White Collar	Blue Collar	Farmer	Total	
1923-1932	25.4	74.6	...	100.0	63
1913-1922	27.8	71.1	1.1	100.0	90
1903-1912	30.8	65.4	3.8	100.0	52
1893-1902	42.1	55.3	2.6	100.0	38
1853-1892	37.8	59.5	2.7	100.0	37

Date of Birth of Respondent	Respondent's Father Farmer				N
	White Collar	Blue Collar	Farmer	Total	
1923-1932	11.4	62.9	25.7	100.0	35
1913-1922	12.5	51.6	35.9	100.0	64
1903-1912	30.6	38.9	30.6	100.1	72
1893-1902	30.4	37.5	32.1	100.0	56
1853-1892	20.7	40.2	39.1	100.0	87

Date of Birth of Respondent	All Categories of Respondent's Father				N
	White Collar	Blue Collar	Farmer	Total	
1923-1932	27.1	65.3	7.6	100.0	118
1913-1922	31.1	56.6	12.2	99.9	196
1903-1912	36.3	48.8	14.9	100.0	168
1893-1902	44.1	39.0	16.9	100.0	118
1853-1892	33.3	42.9	23.8	100.0	147

as mobile is reduced to 28 per cent of the total. The latter nevertheless represents a substantial degree of vertical mobility.

The present analysis is less concerned with mobility in the sample as a whole than with

mobility in the several age cohorts, since it is only from this latter source that conclusions can be drawn concerning *trends*. Thus Table 2 divides respondents into five age cohorts: the first includes all men in their twenties and the last men sixty and over; each of the intermediate cohorts embraces a ten year span.

Uncritical inspection of Table 2 might suggest a notable decline in the opportunities for social advancement among the sons of farmers and of blue collar workers, as well as some increase in the rate of downward mobility among the sons of white collar workers. Actually, however, at least three considerations make such conclusions unwarranted. First, certain demonstrable sampling errors in the data must be corrected. Second, the exclusion of college students from the sample creates a bias which requires correction.⁶ Finally, this table provides no control for the differential effect of *intra-generational* mobility on the several cohorts.

An estimate of the size of the sampling errors can be obtained by comparing the relevant columns of Tables 2 and 3. If there were no such errors, the summary occupational distributions for the five cohorts shown in Table 2 should nearly match the occupational distributions for the universe of American males shown in Table 3. Some minor differences might be expected owing to the two year interval between the 1950 census and the interviewing of respondents and also to the slight difference in definition of the cohorts. Since both sets of figures, however, refer to men of the same age at the time of classification the slight differences in the years of birth should not cause much variation.

Comparison of the figures indicates that the sampling error is rather small in most cells, the most serious occurring in the cases of blue collar and white collar workers in the fourth cohort (that is, those born between 1893 and 1902). In the former case

⁶ Five male students were included in the original sample, but these were not representative of college and university students, since all persons residing in institutional housing facilities were excluded from the sample. Because of the unrepresentative character of this small number of respondents, they were not included in the present analysis.

TABLE 3. OCCUPATIONAL DISTRIBUTION OF AMERICAN MALES IN 1930, 1940, AND 1950, BY DATE OF BIRTH, IN PERCENTAGES (ADAPTED FROM JAFFE AND CARLETON) *

Date of Birth	White Collar Workers			Blue Collar Workers			Farmers			All Workers		
	1930	1940	1950	1930	1940	1950	1930	1940	1950	1930	1940	1950
1921-1930	28.6	58.9	12.5	100.0
1911-1920	23.8	32.1	56.4	56.1	19.9	11.8	100.1	100.0	100.0
1901-1910	24.1	29.1	33.0	54.2	54.6	53.7	21.8	16.3	13.3	100.1	100.0	100.0
1891-1900	28.1	29.3	32.0	52.9	52.6	52.7	19.0	18.1	15.3	100.0	100.0	100.0
1881-1890	27.1	26.8	28.6	50.7	50.5	50.7	22.2	22.7	20.7	100.0	100.0	100.0
1871-1880	26.4	26.5	28.5	45.8	42.2	40.7	27.8	31.3	30.8	100.0	100.0	100.0
1861-1870	24.5	30.6	42.1	29.2	33.4	40.2	100.0	100.0
1851-1860	22.1	35.6	42.2	99.9

* Italicized figures represent the fifth decade of life for each cohort.

the indicated error is approximately 12 per cent, in the latter almost 14 per cent. In other cells the indicated error ranges from 0.1 to 6 per cent. Unless adjustments are made for at least the larger errors, serious misinterpretations of trends could easily occur.

A second difficulty in drawing conclusions concerning trends in inter-generational mobility directly from Table 2 stems from the great expansion of opportunities for higher education. According to the 1950 census approximately 8 per cent of men in their twenties were still enrolled in institutions of higher learning. The majority of such persons are not included in sample surveys since they live in institutional housing facilities such as dormitories and fraternities. Their omission would not be a serious matter if these students were a random cross-section of their age cohort, but this is not the case for they are recruited disproportionately from white collar families and are destined almost to a man for white collar careers. Therefore adjustments must be made in the figures pertaining to the first cohort, if serious errors of interpretation are to be avoided.⁷

The final obstacle blocking efforts to draw conclusions concerning trends in inter-generational mobility directly from Table 2 is created by *intra*-generational mobility and its differential influence on the several cohorts. As careful inspection of Table 3 makes clear, during the period from 1930 to 1950 the percentage of white collar workers

in each cohort steadily increased, with a single exception. This trend was especially evident in the younger cohorts. In the older cohorts, the trend may be largely a product of differential mortality and retirement rates among the several occupations. In the younger cohorts, however, the trend is the result both of a greater volume of upward than of downward mobility and of the educationally delayed entry into the labor force of many white collar workers.

If this pattern continues for the next two decades (the writer sees no reason why it should not), trends in inter-generational mobility on the basis of data gathered at one point in time cannot be determined, unless controls are established for intra-generational mobility and its influence on the occupational distributions of respondents in the several cohorts. Without such controls, and assuming that the net upward shift during the third and fourth decades of life is of appreciable magnitude, the unwarranted conclusion may be drawn that there is less upward mobility and more downward mobility in the younger cohorts than in the older.

The problem of correcting sampling errors was not difficult. As a first step, an attempt was made to estimate the occupational distribution by age cohorts for 1952 by projecting the 1930-1950 trends for each age category (*not cohort*) to 1952. The results of this projection are shown in Table 4. Assuming that the figures in Table 4 represent the true occupational distributions in the cohorts at the time of the sample and, further, that the errors of oversampling and

⁷ A much smaller adjustment is also required for the second cohort.

TABLE 4. ESTIMATED OCCUPATIONAL DISTRIBUTION FOR AGE COHORTS IN 1952, IN PERCENTAGES

Date of Birth	White Collar Workers	Blue Collar Workers	Farmers	Total
1923-1932	29.1	59.4	11.6	100.1
1913-1922	32.5	56.4	11.1	100.0
1903-1912	33.6	54.0	12.4	100.0
1893-1902	32.6	53.4	14.0	100.0
1883-1892	29.0	51.6	19.4	100.0
1873-1882	29.1	41.2	29.7	100.0

undersampling particular categories of respondents were randomly distributed among the several categories of fathers,⁸ a ready basis for correcting sampling errors is provided; solving a series of simple equations is all that is required.⁹

The problem of the omission of college students from the sample was somewhat more difficult to deal with since less adequate data were available on which to base the necessary adjustments. If it is assumed, however, that roughly 8 per cent of the members of the first cohort were excluded from the sample because they were not yet in the labor force, approximately ten individuals must be added to a sample of the size of the first cohort.¹⁰ On the basis of available evidence concerning the occupational origins and careers of undergraduate

⁸ Some assumption is required at this point, and this assumption seems more reasonable than the alternatives.

⁹ The basic equation used in making these adjustments:

$$\frac{\text{percentage of respondents in occupational category in sample cohort}}{\text{percentage of male workers in occupational category in adjusted census cohort}} = \frac{\text{number of respondents in given cell in occupational category in sample cohort}}{X}$$

X represents the numerical value of the cell, after adjustment for oversampling or undersampling.

¹⁰ Census data indicated that 0.5 individuals should be added to the second cohort for this same reason. The addition was classified as a white collar son of a white collar father because children of other than white collar parents, with rare exceptions, continue their education into the fourth decade of life. Moreover, postgraduate education almost always results in white collar employment.

and graduate students, these ten individuals were classified as follows:¹¹

Father's Occupational Category	Individual's Occupational Category After Graduation	N
White Collar	White Collar	6.5
Blue Collar	White Collar	2.5
Farmer	White Collar	0.7
Farmer	Farmer	0.3
	Total	10.0

To meet the problem created by intra-generational mobility required a technique which would permit comparisons among cohorts in terms of a common period in the life cycle. Only in this way could trends in inter-generational mobility be established.

For purposes of the present analysis the fifth decade of life was selected as the basis for comparisons because it seemed that this decade in the careers of the respondents most often and most closely matched the period in their fathers' careers to which respondents referred when asked what their fathers' occupations had been *when they were growing up*. Furthermore, by the fifth decade occupational stability has been achieved in the lives of most men; thereafter there is relatively less intra-generational mobility.

In order to standardize comparisons among cohorts for the fifth decade, it was necessary to devise one procedure for reconstructing the occupational distribution in the two oldest cohorts, and another for predicting the occupational distribution of the two youngest cohorts. The middle cohort required no adjustment.

¹¹ Obviously this distribution involves guesswork. But it is not blind guesswork, as indicated by the following example of this procedure. Statistics from the United States Office of Education revealed that approximately 4 per cent of men receiving their bachelor's degree in 1950 had majored in either agriculture or animal husbandry. Since the great majority of such students are recruited from farm families and intend to make farming their life's work, 3 per cent of the cases added to the sample were defined as farmer's sons who would themselves become farmers. Data employed in making the necessary estimates include: Raymond A. Mulligan, "Socio-Economic Background and College Environment," *American Sociological Review*, 16 (April, 1951), pp. 188-196; Ernest Havemann and Patricia S. West, *They Went to College* (New York: Harper and Bros., 1952); unpublished reports on the University of Michigan student body; census data and reports of the U. S. Office of Education.

Data were available from the censuses of 1930 and 1940 (see Table 3) for reconstructing the occupational distributions of the two oldest cohorts. On the basis of these data it was a relatively simple matter to make the small adjustments in the classification of respondents' occupations required to bring the occupational distribution of the sample into conformity with the distribution for the cohort as a whole in the earlier period.¹²

In the case of the two youngest cohorts, the problems involved were somewhat greater since they required the prediction of *future* occupational distributions. Thanks to data from the last several censuses, however, some basis existed for making such predictions. As inspection of Table 3 reveals, during the period from 1930 to 1950, the percentage of white collar workers among men in the fifth decade of life (see the italicized figures) increased from 27.1 per cent of the total to 33.0 per cent. For farmers, a drastic decline was reported with the figure dropping from 22.2 per cent in 1930 to 13.3 per cent in 1950. A small increase, from 50.7 per cent to 53.7 per cent, was recorded for blue collar workers.

These figures reflect the changing occupational structure of American society, particularly the rapid transformation of agriculture through mechanization and increased capitalization and the somewhat slower

change of urban institutions resulting from the replacement of human power and mechanical skills with other sources of power and machines. Both of these trends have continued unabated and give every evidence of continuing for at least another decade or more.

Thus, one can make a reasonably accurate prediction of the future occupational distributions of the two youngest cohorts on the basis of a simple extrapolation of the trends of the last two decades. Such a prediction incorporates even the retarding effect on structural change of an extended depression, and therefore is not geared to the unduly optimistic assumption of continued prosperity throughout the period in question.

In the writer's view, the farmers pose a special problem. A simple extrapolation might not provide the most accurate prediction since the percentage of farmers is so rapidly approaching the vanishing point. A straight-line projection of the trend of the last two decades yields a prediction of only 2.6 per cent of American males in farming in the fifth decade of life in 1972. Since most social trends taper off in their later stages, as in a normal curve, and since there seems no reason to believe that farming is destined to disappear from the American scene in the foreseeable future, some adjustment seems to be in order. Slowing the predicted decline in the percentage of farmers requires, of course, comparable adjustments in the predicted growth of urban occupations. Estimates for the future occupational distributions of the youngest cohorts, together with the past distributions of the oldest cohorts and the present distribution of the middle cohorts, are shown in Table 5.

If the figures in Table 5 can be accepted as a reasonable estimate of the occupational distributions of the first two cohorts ten and twenty years hence, we then have a basis for ascertaining the volume of intra-generational mobility to be expected. In the case of the first cohort, it appears that the size of the white collar category will increase by 3.8 percentage points as a result of intra-generational mobility, while the blue collar category will increase by 1.6 percentage points. These increases will be offset by a 5.5 percentage point decline in the size of the farmer category. In the case of the

¹² In making these adjustments it was assumed that whatever shifts had occurred were in proportion to the number of individuals in a given cell. Thus the following equation, almost identical to that used in correcting sampling errors, was used to correct for intra-generational mobility in the two older cohorts:

percentage of respondents in occupational category in sample cohort (after adjustment for sampling error and education)	=	number of respondents in occupational category in sample cohort (after adjustment for sampling error and education)
percentage of male workers in occupational category during fifth decade of life, adjusted for minor discrepancy in cohort definition	X	
As in the adjustments for sampling error, corrections were made for the minor discrepancy between the definition of cohorts in the sample and that in the census.		

TABLE 5. OCCUPATIONAL DISTRIBUTIONS OF MALES DURING FIFTH DECADE IN LIFE (RECONSTRUCTED, CURRENT, AND PREDICTED) BY COHORTS, IN PERCENTAGES

Date of Birth	Comparison Year	White Collar Workers	Blue Collar Workers	Farmers	Total
1923-1932	1972	38.2	56.3	5.5	100.0
1913-1922	1962	36.2	55.3	8.5	100.0
1903-1912	1952	33.6	54.0	12.4	100.0
1893-1902	1942	30.0	52.8	17.1	99.9
1873-1892	1932	27.3	50.2	22.5	100.0

second cohort, Table 5 points to a 3.5 percentage point increase in white collar workers, a 1.0 percentage point decline in blue collar workers, and a 2.6 percentage point decline in farmers.

These predictions raise the important question of whether or not such shifts will be affected by the family origins of the men involved. Will blue collar sons of white collar fathers enjoy any advantage over the blue collar or farmer sons of blue collar or farmer fathers in the competition for social advancement? Here, two contradictory lines of reasoning may be stated.

On the one hand, it can be argued that available evidence indicates that with respect to *inter*-generational occupational comparisons, the sons of white collar workers became white collar workers 2.3 times as often as the sons of blue collar workers and 3.5 times as often as the sons of farmers working in urban jobs, in the first two cohorts.¹³ Thus, family origins clearly make a big difference in the chances that a boy has of achieving white collar status in the first decade of adult life, and presumably the influence of family background does not end with the entry of the individual into the labor force. This one might conclude that blue collar sons of white collar workers are more likely to rise through intra-generational mobility than the sons of blue collar workers or farmers. On the other hand, however, it can be argued that those sons of white collar workers who slip into the ranks of blue collar workers are either individually deficient or come from families marginal to the white collar stratum. Accordingly, those who begin their careers in

a blue collar job are individuals who lack special competitive advantages.

Both arguments appear to contain a measure of truth. In the present analysis, therefore, it was assumed that family origins have some effect on the chances of occupational success through intra-generational mobility, but that the effect is only half as great as in the case of inter-generational mobility. In concrete terms, if the probabilities of attaining white collar status through *inter*-generational mobility are in the ratio of 7-3-2 for the sons of white collar workers, blue collar workers, and farmers, respectively, then, for the same categories, the ratio for *intra*-generational mobility is 25-15-12.¹⁴

On the basis of this ratio, a series of equations was worked out to make allowance for the expected shifts in the occupational distributions of the first two cohorts resulting from intra-generational mobility. Table 6 presents estimates (the best the writer is able to make) of the occupational distributions of the five cohorts during their fifth decade of life.

As the reader will note, the adjustments described above result in a very different picture from that indicated by the uncorrected data in Table 2. Table 6 indicates that more of the sons of both white collar and blue collar workers in the youngest cohort will become white collar workers than in any of the previous cohorts. In other words, the rate of downward mobility

¹³ After corrections for sampling errors and delayed entry into the labor force because of higher education.

¹⁴ It may be appropriate to comment here that considerable detail is provided in this paper concerning the assumptions adopted and procedures followed in correcting the raw data shown in Table 2 to enable the reader to evaluate the assumptions for himself and, if he disagrees with them, to make his own calculations of trends based on assumptions of his own choosing.

TABLE 6. EXPECTED OCCUPATIONAL DISTRIBUTION OF AMERICAN MALES IN THE FIFTH DECADE OF LIFE, BY FATHER'S OCCUPATION AND DATE OF BIRTH, IN PERCENTAGES

Date of Birth of Respondent	Father White Collar				Father Blue Collar				Father Farmer			
	Respondent:				Respondent:				Respondent:			
	WC	BC	F	Total	WC	BC	F	Total	WC	BC	F	Total
1923-1932	75.8	24.2	...	100.0	36.2	63.8	...	100.0	15.6	66.4	17.9	99.9
1913-1922	71.2	28.8	...	100.0	32.0	67.3	0.7	100.0	16.1	58.2	25.7	100.0
1903-1912	48.2	50.0	1.8	100.0	27.0	69.9	3.2	100.1	29.3	44.5	26.3	100.1
1893-1902	66.8	28.0	5.2	100.0	27.0	70.5	2.5	100.0	19.9	48.8	31.3	100.0
1873-1892	66.5	33.5	...	100.0	29.4	68.3	2.3	100.0	16.8	46.6	36.5	99.9

from the white collar ranks seems to have declined from earlier decades while the rate of upward mobility from the ranks of blue collar workers seems to have increased. The trend has not been a uniform one for workers' sons, but rather appears to have reversed itself after an earlier period of declining opportunities for upward mobility.

Farmers' sons show exactly the opposite pattern. Here, the percentage entering the white collar category steadily increased until the middle cohort, and then dropped sharply in the most recent cohorts. This decline is especially significant in view of the growing frequency with which farmers' sons leave the land and enter urban occupations. Thus, whereas in the earlier cohorts more than one-quarter of farmers' sons in urban occupations became white collar workers, less than one-fifth of those in the most recent cohort achieved this status.

The two most surprising figures in Table 6 refer to the sons of white collar workers in the third cohort. The percentage of such sons reporting themselves to be white collar workers is much lower in this cohort than in any other. The difference between this figure and others in the same column may be due to some extent, but surely not exclusively, to remaining uncorrected sampling errors. In part at least, the drop in this figure would seem to reflect the influence of the depression of the thirties. Men of this age (17-26 years) were especially vulnerable at the onset of the depression. This age, which includes the years when advanced education and the first fulltime employment are normally obtained, is a critical period in occupational careers. The economic crisis of the 1930s may have deprived large numbers of white collar workers' sons of college ed-

ucation and many may have become established in blue collar jobs from which upward movement later proved impossible. In short, the depression seems to have had a levelling effect on job opportunities within this cohort, equalizing access to the more desirable positions. The validity of this interpretation requires, of course, similar analyses of other national samples.¹⁵

Assuming that Table 2 provides a reasonably accurate measure of the relative proportion of sons of each occupational category in each of the age cohorts after corrections are made for those still in school, and that Table 6 gives an adequate measure of the distribution of these sons among the several occupational categories, it then becomes possible to calculate the volume of upward and downward movement in the total male population. The results of this calculation are shown in Table 7. (Movement between the blue collar and farmer categories is not considered vertical mobility, for reasons cited earlier in this paper.)

As inspection of Table 7 indicates, trends in rates of upward and of downward mobility have been markedly different. The trend line for upward mobility rises slowly but steadily, with a single exception, during the period in question. The trend line for downward mobility, however, approximates an inverted U-curve with a very high peak in the middle. This capacity of the two trends to vary independently is a feature of change in systems of social stratification that has not thus far received the attention it deserves.

¹⁵ Several attempts were made by the writer to secure permission to analyze other national samples. Unfortunately, however, those controlling the data essential for the analysis did not grant this request.

TABLE 7. ESTIMATED PERCENTAGE OF MALES UPWARDLY MOBILE, DOWNWARDLY MOBILE, AND NON-MOBILE ACCORDING TO INTER-GENERATIONAL OCCUPATIONAL MOBILITY, BY COHORTS.

Date of Birth	Upwardly Mobile	Non-Mobile	Downwardly Mobile	Total	UM-DM *
1923-1932	22.9	72.1	5.0	100.0	17.9
1913-1922	19.9	73.9	6.2	100.0	13.7
1903-1912	20.9	65.5	13.6	100.0	7.3
1893-1902	18.1	75.1	6.8	100.0	11.3
1873-1892	17.4	77.4	5.2	100.0	12.2

* Upwardly mobile minus downwardly mobile, that is, net upward mobility.

The rise in the rate of upward mobility shown in Table 7 is a trend easily subject to misinterpretation. This rise may be attributed to the *combined* influence of only two factors: the expanding proportion of white collar jobs in the American economy; and the declining ratio of farmers' sons to blue collar workers' sons. Were *either* of these two factors eliminated, the margin of difference in the percentage of upwardly mobile males between the first and last cohorts would be virtually wiped out. Thus, if no increase occurred in the proportion of white collar jobs available to men in the fifth decade of life between 1932 and 1972, only 17.5 per cent of the members of the latest cohort would be upwardly mobile as contrasted with 17.4 per cent of the earliest cohort.¹⁶ Similarly, if there had been no shift in the ratio of the number of farmers' sons to blue collar workers' sons during this period, only 18.4 per cent of the members of the latest cohort, as contrasted with 17.4 per cent of the earliest, would be upwardly mobile. In the absence of both of these factors, a rather marked decline in the percentage of upwardly mobile males, from 17.4 per cent in the earliest cohort to 13.1 per cent in the latest, would have taken place. In other words, the combined effect of all other factors contributing to upward movement in the population appears to have been insufficient to maintain the rate of upward movement at the level attained by the earliest of the five cohorts.

A somewhat similar situation prevails in the case of downward mobility. If the influence of the declining proportion of blue collar jobs in the economy is controlled, one finds, in fact, a slight increase in the

percentage of white collar workers' sons who are downwardly mobile. Comparing the earliest and latest cohorts, the percentage of downwardly mobile persons in the sample would have increased from 5.2 to 5.9 per cent. These figures indicate the tremendous importance of changes in the occupational structure for both the increase in the rate of upward mobility and the reduction or persistence of the rate of downward mobility.

CONCLUSIONS

In the last several years a number of sociologists have surveyed the accumulated literature on vertical mobility in American society in an effort to determine whether the rate has increased or decreased in modern times. The more critical writers have generally agreed that only the most tentative conclusions can be made concerning trends because of the absence of adequate data; and one of them (Chinoy) has even denied that such conclusions are as yet warranted.¹⁷

The present study by no means provides an answer to the question of whether the rate of vertical mobility in American society is increasing or decreasing. The writer shares the increasingly accepted view that the conception of a single rate is unprofitable, except in making the crudest of comparisons. The aim of this study is simply to provide empirical data concerning trends with respect to two specific forms of vertical mobility—upward and downward inter-generational occupational mobility. These were selected because of the availability of the data and the importance, presumably, of

¹⁶ The method employed here is essentially the same as that employed by Rogoff, *op. cit.*, in her study of Marion County.

¹⁷ Ely Chinoy, "Social Mobility Trends in the United States," *American Sociological Review*, 20 (April, 1955), pp. 180-186; cf. Barber, *op. cit.*, p. 468; Kurt B. Mayer, *Class and Society*, New York: Random House, 1955, p. 75.

the variables. It should not be assumed, however, that the trends depicted here are typical of trends in other forms of vertical mobility in American society.

The findings of this study are based on a single, relatively small sample, and a cohort analysis substitutes for the preferred time series form of analysis. Therefore caution must be exercised in drawing conclusions. However, in view of the absence of time series data and of national samples analyzed for *trends*, the findings of the present study, the writer believes, are at least suggestive. The major findings may be summarized:

1. During the first half of the present century, the rate of upward inter-generational mobility into white collar occupations seems to have increased, with this increase being almost wholly attributable to the expansion in the proportion of white collar jobs and the declining proportion of farmers' sons in the lower stratum.

2. During the same period, the rate of downward inter-generational occupational mobility seems to have risen somewhat and then declined, with the rate at the end of the period corresponding roughly to that at the beginning. The rate of downward mobility would probably have increased somewhat over the total period except for the decline in the proportion of lower status jobs.

3. The relative chances of farmers' sons attaining white collar status has declined appreciably in comparison with both blue collar and white collar workers' sons.¹⁸

¹⁸In a recent paper Lipset reports that chances for occupational success vary directly with the size of the community in which the individual was raised. Although his study does not deal with trends over time, one might infer from his findings that, assuming progressive urbanization, the life-chances of farmers' sons in the job market will continue to deteriorate. Seymour M. Lipset, "Social Mobility and Urbanization," *Rural Sociology*, 20 (September–December, 1955), pp. 220–228. Another hypothesis needing exploration is that recent cohorts

These conclusions do not appear to conflict significantly with the findings of earlier trend studies of more limited segments of the American population. For example, the data from Rogoff's investigation of Marion County, Indiana, when classified in terms of the three basic occupational categories employed in the present study, yield very similar results, if adjustments are made for the underrepresentation of farmers in her samples as well as the fact that the individuals classified are mainly in their twenties and thus will experience some net upward movement before they reach the fifth decade of life.¹⁹ Similarly, no important inconsistencies emerge when the present findings are compared with those of studies of the social origins of business elites, if proper allowance is made for the fact that, at the time of study, business elites are generally beyond the fifth decade in life.²⁰ In summary, then, the findings of this study tend to confirm certain trends suggested in earlier investigations of limited segments of the American population. In addition, they suggest trends which have not previously been noted. In this latter connection, the "independent" variation of the trend lines for upward and downward mobility and the relative decline in the chances of farmers' sons attaining white collar status seem especially noteworthy.

of farmers' sons have included a larger proportion of Negroes, and thus the decline in mobility opportunities is a function of race rather than occupational origin.

¹⁹Rogoff, *op. cit.*

²⁰W. Lloyd Warner and James C. Abegglen, *Occupational Mobility in American Business and Industry, 1928–1952*, Minneapolis: University of Minnesota Press, 1955; Mabel Newcomer, *The Big Business Executive*, New York: Columbia University Press, 1955; Suzanne Keller, *The Social Origins and Career Lines of Three Generations of American Business Leaders*, unpublished Ph.D. dissertation, Columbia University, 1953.

THE DIFFERENTIAL POLITICAL ACTIVITY OF PARTICIPANTS IN A VOLUNTARY ASSOCIATION *

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THE relationship between the democratic state and other associations of its citizenry has been the subject of theorizing and philosophizing from the very beginnings of modern democratic thought in the 17th century. The early discussion focused primarily on sovereignty and natural rights—was the state the supreme sovereign association, or did it share sovereignty with other groups, particularly religious bodies? But there was also the question of desirability—was it desirable for sovereignty to be shared among several kinds of associations?

Two western democratic traditions developed on this point. One, stemming from Rousseau and the French Revolution, envisions secondary associations as divisive forces, endangering equality and detracting from allegiance to the democratic state by promoting conflicting loyalties. The second, stemming from the Puritan Revolution and John Locke, sees secondary associations as cohesive forces, promoting liberty and the democratic society by serving as intermediate sources of influence between the individual and the state.¹

Tocqueville, an adherent to the latter point of view, is an early source for the subsidiary thesis that secondary associations also serve the important function of drawing

their participants into the life of the general society:

The greater the multiplicity of small affairs, the more do men, even without knowing it, acquire facility in prosecuting great undertakings in common.

Civil associations, therefore, facilitate political association. . .²

Much later, Durkheim pursued the same theme in considering the functional importance of occupational groups:

A [democratic] nation can be maintained only if, between the State and the individual, there is intercalated a whole series of secondary groups near enough to the individuals to attract them strongly in their sphere of action and drag them, in this way, into the general torrent of social life.³

Restated in terms of voluntary associations and the political arena, the thesis expressed by these writers is the focus of this paper: voluntary associations are stimuli which rouse their participants to greater involvement in the political life of the general society.⁴

It should be clear that the thesis specifies

² Alexis de Tocqueville, *Democracy in America*, translated by Phillips Bradley, New York: Vintage Books, 1955, Vol. 2, p. 123.

³ Emile Durkheim, *The Division of Labor in Society*, translated by George Simpson, Glencoe: Free Press, 1947, Preface to the 2nd edition, p. 28.

⁴ The distinguishing characteristics of the voluntary association are that it be private, non-profit, voluntary in that entrance rests on mutual consent while exit is at the will of either party, and formal in that there are offices to be filled in accordance with stipulated rules. These traits serve to differentiate the voluntary association from public and governmental bodies; profit-making corporations and partnerships; family, clan, church, nation and other groups into which the individual is born; informal friendship groups, cliques, or gangs. This is an amended version of the definition contained in *Our Cities, Their Role in the National Economy*, Report of the Urbanism Committee to the National Resources Committee, Washington, D. C.: U. S. Government Printing Office, 1937, p. 24.

* Revised version of paper read at the annual meeting of the American Sociological Society, Washington, D. C., August, 1957.

¹ For brief expositions bearing on this point, see George H. Sabine, "The Two Democratic Traditions," *Philosophical Review*, 61 (October, 1952), pp. 451-474; Robert A. Horn, *Groups and the Constitution*, Stanford: Stanford University Press, 1956, Chapter 2; Harold J. Laski, "Freedom of Association," in *Encyclopedia of the Social Sciences*, New York: Macmillan, 1934, Vol. 6, pp. 447-450; Francis W. Coker, "Pluralism," *ibid.*, Vol. 12, pp. 170-174; Robert A. Nisbet, *The Quest for Community*, New York: Oxford University Press, 1953, Chapter 7.

a dynamic relationship in which participation in voluntary associations is the independent variable, political activity the dependent variable, with the general society the context for both.

In the past thirty years several studies have provided data indicating static relationships between these two variables. Dennis⁵ showed that members of the Lansing (Michigan) chapter of the Daughters of the American Revolution were more likely to have registered prior to the 1928 presidential election than the women in either a random sample of the community, or the women in a sample of their immediate neighbors. Freedman and Axelrod⁶ reported that the higher the level of formal group participation claimed by Detroit residents, the more likely they were to state that they had voted in national, state, and local elections. Hastings⁷ found in Pittsfield (Massachusetts) that persons indicating membership in voluntary associations were more likely to state that they voted regularly than were non-members, and that those indicating membership in some kinds of voluntary associations were more likely to make this claim than were others. Lazarsfeld, Berelson, and their associates showed in Erie County (Ohio)⁸ and Elmira (New York),⁹ respectively, that the tendency for persons on any particular socio-economic status level to vote for the same political party was most prominent among those belonging to voluntary associations. Agger and Ostrom¹⁰ found that, as reported by

⁵ Wayne Dennis, "Registration and Voting in a Patriotic Organization," *Journal of Social Psychology*, 1 (May, 1930), pp. 317-318.

⁶ Ronald Freedman and Morris Axelrod, "Who Belongs to What in a Great Metropolis," *Adult Leadership*, 2 (November, 1952), pp. 6-9.

⁷ Philip K. Hastings, "The Non-Voter in 1952: A Study of Pittsfield, Massachusetts," *Journal of Psychology*, 38 (Fall, 1954), pp. 301-312.

⁸ Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice*, New York: Columbia University Press, 1948, pp. 145-147.

⁹ Bernard Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting*, Chicago: The University of Chicago Press, 1954, pp. 46-53.

¹⁰ Robert E. Agger and Vincent Ostrom, "The Political Structure of a Small Community," *Public Opinion Quarterly*, 20 (Spring, 1956), pp. 81-89. Also see Robert E. Agger, "Power Attributions in the Local Community: Theoretical and Research

inhabitants of a rural Oregon community, differences in kind of political involvement were related to differences in extent of membership in formal organizations. Wright and Hyman¹¹ noted that Denver residents reporting membership in voluntary associations indicated greater involvement in civic affairs than did others.

Although the data in each of these cases describe a static situation, the evidence lends itself to the imputation that these are dynamic relationships in which participation in voluntary associations is the causal factor.¹² The imputation would be strengthened were there data about situations in which, for example, participants of voluntary associations were shown to have increased their political activity relative to non-participants. This requires measurement of political activity at a minimum of two points in time: one prior to or early in, the other subsequent to or late in, the period of affiliation.

Lipset and his associates¹³ implicitly followed this line of reasoning in their recent study of the Typographical Union, showing that those persons who reported active participation in the union's social groups—in contrast to those who did not—subsequently demonstrated increased knowledge about, and interest and involvement in, the internal politics of the union. These data, together with evidence describing the union environment, gave strong support to the conclusion that participation in the union's social groups had caused the differential change.

As yet there are no reported cases detailing comparable dynamic relationships within other labor groups, or within other voluntary associations. Furthermore, there remains the question of whether a relationship within a

Considerations," *Social Forces*, 34 (May, 1956), pp. 322-331.

¹¹ Charles R. Wright and Herbert H. Hyman, "Voluntary Association Memberships of American Adults: Evidence from National Sample Surveys," *American Sociological Review*, 23 (June, 1958), pp. 284-294.

¹² For example, the imputation is made very explicit by Berelson, *et al.*, *op. cit.*; and by Seymour M. Lipset, "Political Sociology, 1945-55," in Hans L. Zetterberg, editor, *Sociology in the United States of America*, Paris: UNESCO, 1956, p. 47.

¹³ Seymour M. Lipset, Martin Trow, and James Coleman, *Union Democracy*, Glencoe, Ill.: Free Press, 1956.

"small society" (in this instance a local of the Typographical Union) is duplicated within the larger society. That is, does participation in voluntary associations cause the individuals concerned to increase their political activity in the *community at large*?

The present paper describes the results of an inquiry bearing specifically on this point. The particular organization examined is the Warren County-Front Royal Recreation Association, Inc. (Virginia)—an action group formed for the sole purpose of taking direct steps to meet the community's need for a publicly sponsored recreation program. While clearly analysis of this one case cannot go very far in answering the focal question posed above, it does supply a type of evidence not currently available.¹⁴

The data and analysis center on five subsidiary questions:

- (1) With respect to political activity, who are the people who became participants in the Association? (Static dimension.)
- (2) What changes, if any, occurred in their political activity subsequent to their participation in the Association? (Dynamic dimension.)
- (3) Are there variations in political activity (both static and dynamic dimensions) related to differences in kind and extent of participation in the Association?
- (4) If changes in political activity occurred, to what extent can they be attributed to the influence of the Association?
- (5) What are the social processes and psychological mechanisms involved in any indicated relationships?

The facts of participation in both the Recreation Association and in public affairs are based on established records—they are not self-reported.¹⁵ Data for the study were collected in the period 1949–1951.

¹⁴ The data for this case are based on work reported in Herbert Macoby, "The Relationship between Participation in the Community Self-Help Association and Participation in Political Activity," microfilmed Ph.D. dissertation (Publication No. 13,982 of University Microfilms), Columbia University, 1955.

¹⁵ With the exception of the Dennis study, *op. cit.*, the research data previously cited rely on self-reported information. What these studies show is that people who say that they engage in political activity are likely to say that they belong to voluntary associations. There remains the question of reliability: to what extent is the self-reported information a reflection of fact or fancy? Is there a relationship between the acts the individuals per-

THE COMMUNITY AND THE ASSOCIATION

Warren County is in northern Virginia. From 1936–1946, under the impact of tourist trade and industrialization, the county changed from a largely rural farm area into a predominantly urban community. By 1950 the population of the county was almost 15,000—77 per cent greater than in 1930. Front Royal, the county seat, grew correspondingly from less than 2,500 to more than 8,000 inhabitants.

The rapid economic change and growth created problems, one of which was inadequate recreational facilities. In response to this need, a citizens' group was incorporated in 1948 as the Warren County-Front Royal Recreation Association. During its first years (1948–1950), it secured the direct support of almost 1,000 adults from all sections of the community, built and equipped twelve summer playgrounds and a stadium, and was providing professional supervision for a wide range of recreational activities.

PARTICIPATION AND POLITICAL ACTIVITY

The analysis focuses on those adults who in the period 1948–1949 voluntarily affiliated themselves with the Association by contributing one or more of the following: money, property, personal service. Individuals who contributed money, property, or both, but not personal service, are here called *financial participants*. Individuals who were of personal service, whether or not they also contributed money, property, or both are called *service participants*.¹⁶ Service participants are further divided into high actives and low

form in addition to any relationship between the acts they say they perform?

¹⁶ We are concerned here only with those persons who supported the Association, not those (primarily children and adolescents) who took part in its recreational activities. Contributions of money were made by individual sponsors as one dollar "membership fees," or as purchases of non-interest-bearing Recreation Association Bonds. Personal services included: serving on the Board of Directors, taking part in organizational and planning meetings, canvassing for members and funds, selling Recreation Association Bonds, helping with special fund-raising events, working as a volunteer recreation supervisor, clearing land for playgrounds, erecting playground equipment, doing volunteer clerical work.

actives.¹⁷ When grouped together, financial participants and service participants are called *participants*, while all others are called *non-participants*.

The analysis is based on voting statistics for the 1949 primary election and the 1951 general election,¹⁸ for persons living in Front Royal North only since accurate voting data on individuals were made available for this one of the two districts into which Front Royal is divided.¹⁹ However, insofar as comparable analyses can be made based on poll tax eligibility for all of Warren County, and on registration for all of Front Royal, the same general results obtain.²⁰

¹⁷ The basis for this division are estimates made by three officers of the Association, each of whom rated independently the extent of participation of each service participant about whose activities in the Association he knew. An arbitrary six-point scale from 0 to 5 was used. Ratings for any one participant were averaged. The resulting categories are combined as follows: 0-0.9 (low actives); 1.0-5 (high actives). For the service participants rated by each of the three judges, the correlations between pairs of judges are .742, .623, and .614. The estimated reliability coefficient for the sum (or average) rating by the three judges is .856. See Maccoby, *op. cit.*, pp. 137-140.

¹⁸ For comparative purposes, it was important that one election had taken place prior to or early in the formation of the Association and the other several years later. The particular elections used are the only ones meeting this requirement for which complete voting statistics were made available. In each election, both state and local offices were at issue.

¹⁹ Insofar as the writer could determine from talks with residents, including a real estate agent, there are no distinct differences in the kind of people between those who live in Front Royal North and Front Royal South. Comparative census data are not available since these two precincts do not correspond to census districts.

²⁰ Needless to say, there is much more to political activity than payment of poll taxes, registration, or voting. However, by seeking an index based on established records rather than on self-reported facts (see footnote 15), the writer was limited in choice to these three. Since voting is the presumed end of registration and poll tax, it is a logical selection. Furthermore, focusing on voting rather than on poll tax, or registration, or on all three in combination, simplified data presentation and eliminated confusing barriers for explanation and interpretation of results. This will be apparent in the discussion of structural conditions bearing on voting participation in Virginia. The relationship between other kinds of political activity (self-reported) and participation in the Association are presented as supplementary evidence.

Static Dimension

The 1949 capitation list for Warren County includes the names of 1,287 individuals in Front Royal North who met poll tax eligibility requirements for voting in that year. Two hundred and eighty-six of them were affiliated with the Recreation Association—146 as financial participants and 140 as service participants—while 1,001 were non-participants. Of the 140 service participants, 70 were high actives and 70 were low actives.

As the data in Table 1 show, a disproportionately large part of the group who participated in the Association came from among those in the community who were politically active. Approximately two of every three Association participants (68 per cent) voted in the 1949 primary election, whereas only one of every two non-participants (49 per cent) voted.

TABLE 1. VOTING IN THE 1949 PRIMARY ELECTION
IN FRONT ROYAL NORTH BY 1,287 PERSONS:
CLASSIFIED BY AFFILIATION WITH THE
RECREATION ASSOCIATION

Affiliation	Number Meeting Poll Tax Eligibility Requirements	Number Voting	Per Cent Voting
Non-participants	1001	495	49
Participants	286	193	68
Financial	146	97	66
Service	140	96	69
Low Actives	70	43	61
High Actives	70	53	76
Total	1287	688	54

With respect to kind of participation, there is little difference in voting between service participants and financial participants—69 per cent of the former having cast ballots and 66 per cent of the latter. With respect to extent of participation in the Association, 76 per cent of the high actives voted, compared to 61 per cent of the low actives.²¹

²¹ Similar comparisons for the 1951 general election show an identical pattern of results: 74 per cent of Association participants voted, 43 per cent of unaffiliated persons, 76 per cent of service participants, 72 per cent of financial participants, 81 per cent of high actives, 69 per cent of low actives.

TABLE 2. CHANGE IN VOTING IN FRONT ROYAL NORTH FROM 1949 TO 1951:
CLASSIFIED BY AFFILIATION WITH THE RECREATION ASSOCIATION

Affiliation	Non-Voters in 1949				Voters in 1949			
			"Voting" in 1951				"Not Voting" in 1951	
	Number	Number	Per Cent	Number	Number	Per Cent	Number	Per Cent
Non-participants	347	134	39	382	103	27		
Participants	77	51	66	158	32	20		
Financial	39	23	59	77	17	22		
Service	38	28	74	81	15	19		
Low Actives	23	16	70	35	6	17		
High Actives	15	12	80	46	9	20		
Total	424	185	44	540	135	25		

Dynamic Dimension

The analysis now concerns only those 964 persons who have their names entered on the capitation lists for Front Royal North in 1949 and 1951, indicating that they met poll tax eligibility requirements for voting in both years. Of the 964, 119 were service participants, 116 were financial participants, and 729 were unaffiliated.

Four alternative combinations of voting activity were possible for each person: voting in both elections; voting in the first election but not in the second; voting in the second election but not in the first; voting in neither election. The analysis focuses on the second and third alternatives, which represent patterns of changed political behavior.

As Table 2 suggests, the percentages of both participants and non-participants who voted are larger for 1951 than for 1949. Among those who failed to vote in 1949, however, Association participants were much more likely to vote in 1951 than were non-participants. Of every three non-voters among participants, two became voters; of every three non-voters among non-participants only one became a voter. Conversely, of those who voted in the first election, 20 per cent of the participants failed to vote in the second, in contrast to 27 per cent of the non-participants.

With respect to kind of participation, non-voters among service participants were much more likely to become voters than were non-voters among financial participants (74 per cent and 59 per cent, respectively). Failure to vote in the second election, how-

ever, was about the same among first election voters who were service participants as among those who were financial participants (19 and 22 per cent, respectively).

With respect to extent of participation, non-voters among the high actives were somewhat more likely to become voters than were non-voters among the low actives (80 per cent and 70 per cent, respectively). However, failure to vote in the second election was about the same among first election voters whether they were high or low actives (20 per cent and 17 per cent, respectively).²²

DISCUSSION

It is quite clear from these data that Association participants, as a group, were politically different from other persons in the community in at least three respects: they were much more likely to be voters; they were somewhat more likely to remain voters; and they were much more likely to become voters if they had been non-voters. Furthermore, service participants tended to differ from non-participants somewhat

²² Statistics for the two combinations of voting activity not discussed in the text complement these data. Thus, 54 per cent of Association participants were regular voters (i.e., voted in both 1949 and 1951) compared to 38 per cent of non-participants; 55 per cent of the service participants compared to 50 per cent of the financial participants; 61 per cent of the high actives compared to 50 per cent of the low actives. Conversely, only 11 per cent of participants failed to vote in both elections in contrast to 29 per cent of non-participants; 8 per cent of service participants in contrast to 14 per cent of financial participants; and 5 per cent of high actives in contrast to 12 per cent of low actives.

more in these respects than financial participants, and, similarly, high actives tended to differ from non-participants somewhat more than did low actives.

Why the differential in political activity?

So many studies have pointed to the correlation of political activity with characteristics like sex and race that it is perhaps important to take note of the possibility that such empirical regularities also apply to the relationships existing in this case study. There is no doubt, for example, that Association participants over-represented the male, white, middle class, and middle-aged elements of the community—qualities associated with greater voting in the general society. With respect to sex and race, the imbalance is not related to differential political activity in this instance.²³ Complete data on other social traits (for example, education, religion, socio-economic status) which might be related to the differential voting rates could not be obtained from available sources. In any case, however, correlations between political activity and these broad social characteristics provide no fundamental explanation, leaving unspecified underlying reasons for the relationships. They become meaningful when seen in connection with social structure and predispositions.²⁴

The structural conditions which affect voting in Virginia most directly are built into the state's political system. Poll tax and registration requirements²⁵ inhibit politi-

tical activity. None the less, they cannot, or do not, adequately account for the particular pattern of results obtained in this study. Poll tax was eliminated entirely as an explanatory factor by including only those persons who met poll tax eligibility requirements. In the post-World War II period, the provision for permanent registration, the almost 100 per cent literacy in Front Royal, and the assistance and civil treatment registrars accord all applicants make it less likely than in earlier years that the registration requirement will discourage prospective voters. In any event, since there was no change in election procedures and requirements between 1949 and 1951, registration can hardly account for the differential change in voting rates between those two years.

One further point relevant to the structure of the political system should be considered. There are sections of the South in which some persons prefer not to identify themselves as Republicans, and so avoid voting in primary elections where they must declare party affiliation. To some extent this is still true in Warren County—Republican candidates regularly draw a larger proportion of the vote cast in general elections than they do in primary elections. If Association participants, service participants, and high actives had a considerably larger proportion of Republicans than the groups with which they were compared, this might account for the differential voting rates. While data on party affiliation for individuals are not available, other evidence makes this possibility appear highly unlikely. As already noted (in footnote 21), the same pattern of differential voting activity obtained in both a primary and a general election. Moreover, the proportion of persons voting in the 1951 general election but not voting in the 1949 primary election (the reverse of the approach in Table 2) is either smaller for Association participants, service participants, and high actives than for their respective comparison groups, or very similar.²⁶ The opposite would be ex-

²³ For example, men, women, and Negroes who participated in the Association were substantially more active politically than their counterparts among non-participants. For participants, 67 per cent of the men voted in the 1949 election, 68 per cent of the women, and 67 per cent of Negroes. Comparable percentages for non-participants are 55, 53, and 35, respectively.

²⁴ On this point see the excellent statement by Seymour M. Lipset, Paul F. Lazarsfeld, Allen H. Barton, and Juan Linz, "The Psychology of Voting: An Analysis of Political Behavior," in Gardner Lindzey, editor, *Handbook of Social Psychology*, Cambridge, Mass.: Addison-Wesley, 1954, Vol. 2, Chapter 30, particularly pp. 1126-1150.

²⁵ Virginia law requires of each adult citizen seeking to vote that he (1) be registered and (2) pay, at least six months prior to the particular election, all poll taxes assessed against him for the three years preceding that election. In order to register he must (1) pay any poll taxes assessed against him during the preceding three years and

(2) commit to memory all required information since he is not supposed to have any written or oral assistance in completing his application form. *Virginia Election Laws*, Richmond: Secretary, State Board of Elections, 1948, pp. 5-6.

²⁶ Twenty-nine per cent for Association participants, 32 per cent for unaffiliated persons, 30 per cent for service participants, 28 per cent for fi-

pected were covert Republicanism to be a significant factor.

It appears improbable, then, that these particular conditions account for the voting relationships noted in this study. Furthermore, the writer knows of no other political system arrangements which are relevant here.

There is, however, evidence from interviews with a random sample of 24 Association participants which suggests that there may have been important differences in predisposition behind the relationships. Thus, more extensive participation in the Association (high actives)—in contrast to minimal participation (low actives)—tends to be linked to: (1) positive and non-fatalistic sentiments toward the social environment; (2) a role definition of the citizen in a democracy in political terms; (3) interest and activity in public affairs, but not "getting worked up" about them; (4) participation in political and civic organizations; and (5) parents who consider(ed) voting important.²⁷

Similarly, political activity is linked to these very predispositions in the same way, holding constant extent of participation in the Association. Since low actives are most similar to non-participants both with respect to their participation in the Association and their political activity, it would appear likely that parallel predispositional differences may

nancial participants, 25 per cent for high actives, 36 per cent for low actives.

²⁷ Of the 24 interviewees, 15 were high actives and nine were low actives. The response of each interviewee to questions which the writer thought might bear on differential political activity was rated on a five-point scale. A percentage score was derived for each group on each question or series of questions by dividing the total number of rating points individuals in the group received by the total number of points possible. For the five particular differences considered in the text, the percentage scores are as follows for the high actives and the low actives, respectively: (1) 78 and 61; (2) 56 and 35; (3) 63 and 47; (4) 65 and 49; (5) 81 and 65. There are parallel, but smaller, differences with respect to other characteristics and attitudes not mentioned in the text—e.g., their opinions as to whether voting does any good (69 and 60, respectively), whether their friends are interested in public affairs (74 and 63, respectively), whether voting is important to their friends (86 and 75, respectively). See Maccoby, *op. cit.*, Appendix D, pp. 190-194, for the interview schedule.

generally distinguish participants from non-participants.

Assuming the relevance of differences in socio-psychological predispositions for an explanation of differential voting activity, we still need to determine the processes by which persons with these particular predispositions were activated and directed to participation in the Association, and, in the case of those who changed in their political activity, the processes which stimulated the change.

Static Dimension

Recruitment procedure was probably an important factor behind the differential in political activity. First, self-recruits to the Association in all likelihood were highly motivated toward civic participation and predisposed to political activity. Confirming examples are the persons who initiated the drive which resulted in the formation of the Recreation Association: they were leaders of important voluntary associations in the community, and they were voters. Secondly, the Association secured most of its individual supporters, particularly service participants, through the efforts of existing community organizations whose natural field of recruitment was their own memberships.²⁸ In a few instances, whole organizations were coopted for tasks which the Association assigned. This assistance was one of the important by-products of other community groups formally holding positions of power in the Association through their representatives on the Association's Board of Directors.²⁹

A second by-product of this structural arrangement was that high actives in the

²⁸ Many financial participants were also secured in this way. However, it is likely that proportionately fewer of them were affiliated with other voluntary associations because, in contrast to service contributions, money contributions were solicited by canvassing the whole community.

²⁹ During the initial period with which we are concerned, each of the 50 affiliated groups was represented on the Association's Board of Directors by its appointed delegate. Since organizational delegates were the dominant group on the Board, control of the Association was nominally vested in the already established groups of the community—even though in practice members of the Board served as individuals.

Association were likely to be high actives in the constituent organizations. On the one hand, being a Board member of the Association was to be in a position conducive to greater participation and, on the other, appointees to the Board were usually persons selected from among the more active members of the constituent groups. In effect, in seeking participants, the Association addressed itself by and large to a special population, consisting of persons with demonstrated interest in group activity and community affairs—the very people who are most likely to be voters.³⁰

This differential recruitment helps to explain the differential political activity of Association participants at any given time.

Dynamic Dimension

But why did politically inactive Association participants become voters to a greater extent than politically inactive non-participants? What were the factors involved? What were the mediating processes? Was participation in the Association incidental or instrumental in this differential change?

Taking predispositional and structural factors as given, two basic interpretations have been suggested as underlying the various specific explanations for observed variations in voting rates.³¹ In one, voting is viewed as an attempt to influence the policies of government. In the other, voting is seen as a response to perceived social pressures which require the individual to vote, regardless of whether he feels that any particular policies will affect him.

In good conscience we can rule out the possibility that the Association was in any direct way involved in inducing its participants to influence governmental policies. The Association took no active part in election politics. There was no direct tie between election issues and Association objectives, or between election personalities and Association participants; furthermore, interviews showed conclusively that participants perceived no such connection.

³⁰ Freedman and Axelrod, *op. cit.*, p. 9; Hastings, *op. cit.*, Table 1, p. 303; Agger and Ostrom, *op. cit.*, Table 1, p. 88; Wright and Hyman, *op. cit.*, Table 6, p. 293.

³¹ Lipset et al., "Psychology of Voting . . ." *op. cit.*, p. 1128.

With respect to the second interpretation, it has been suggested that contact between persons who are politically active and those who are not "increases the possibility that the latter will be politically stirred."³² It is difficult of course to assess empirically either the amount and quality of such contacts or the relevance of a particular set of contacts for political change. However, two corollaries logically follow from this particular thesis and provide grounds on which the contention of Association effect on political activity can be based. Other things being equal, it should hold that: (1) as between two groups, the one with the larger proportion of political actives may be expected to have more effect on its political inactives; and (2) within a group, those political inactives who participate most in the group are more likely to be affected than those who participate least. The facts of this case study clearly fit the corollaries.

Nevertheless, the argument weakens considerably when important factors hidden in the phrase "other things being equal" are considered, for example, the absolute amount and the quality of social contact within the Association. Unlike the Typographical Union, with respect to which the social contact thesis was elaborated, there was no occupational community in the Recreation Association, little discussion of politics, and for only the top leadership of no more than 15 persons was there continuous personal contact within the framework of the Association. Since acceptance of a reference group does not necessarily depend on contact or amount of contact, it is conceivable that even minimal interaction between voters and non-voters in an organization like the Recreation Association can have some effect in activating or even changing the latent predispositions of non-voters. But such an explanation by itself is unsatisfying, particularly because of the few politically inactive people who were at the same time deeply involved in the Association.

A stronger case can be made for the thesis of social contact if participation in the Association is taken as being only incidental to the changed activity. As noted above, most participants in the Association were

³² Lipset et al., *Union Democracy*, p. 98.

recruited through other organizations. Consequently, their activity on behalf of the Association was mediated through such groups as the Chamber of Commerce, the Parent-Teachers' Association, the American Legion, the King's Daughters, and the Loyal Order of Moose. While their contacts with other persons in the Association's interaction system may have been minor, their contacts with these same participants in other interaction systems were probably greater. Furthermore, in these other interaction systems they had contacts with politically active persons who were not participants in the Association. Thus, in effect, this interpretation would have it that to a considerable degree the increased political activity of Association participants is probably the reflected result of their participation in these other social systems. The increase in political activity is then greater for Association participants than for non-participants because the former were more likely to be involved in these other social systems. The differentials in increased political activity *among* participants can be explained similarly.

Admittedly, this is speculation. But the facts available support the plausibility of the argument presented here. The alternative interpretation is that particular societal events, forces, or conditions (other than those already considered and rejected) affected Association participants differentially as members of the larger community—and so differentially affected their interest in influencing governmental policies through the polls. The writer has no information to substantiate this alternative view.

SUMMARY

From the beginnings of modern democratic thought in the 17th century, the important bearing of the voluntary association on the existence of democratic society has been recognized. One of its positive influences, as suggested by Tocqueville and Durkheim, is the stimulation of participants to greater involvement in the life of the gen-

eral society. A case study of one voluntary association provided opportunity to examine this possibility empirically and to consider both the conditions under which the posited effect might occur and the processes involved in such stimulation.

The study showed that participation in this one voluntary association is related to political activity in both its static and dynamic dimensions: participants were more likely to be voters than were non-participants, they were more likely to remain voters, and they were more likely to become voters if they had been non-voters. Furthermore, this same pattern of differential political activity generally distinguished service participants from financial participants and the more active participants from the less active ones. Evidence from the study supports the plausibility of the following explanation for this pattern:

- (1) Because the objective of the association was community action, the persons attracted to it were fundamentally predisposed to participate in community affairs in general, including political activity in particular.
- (2) The process of differential attraction was supported by a recruitment procedure which emphasized prior participation in other community organizations.
- (3) Participants were in contact with highly concentrated groups of politically predisposed and politically active persons, both within the particular association, and more especially within other organizations to which they belonged.
- (4) These contacts with voters tended to activate or reinforce those latent predispositions of participants which were favorable to political involvement.
- (5) This same process affected non-participants proportionately less because they were both less predisposed to political involvement and in less contact with politically active persons.

Evidence also supports the plausibility of comparable explanations for the differences in political activity between service participants and financial participants, and between high actives and low actives.

PUBLIC HEALTH AS A CAREER OF MEDICINE: SECONDARY CHOICE WITHIN A PROFESSION *

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ONE of the striking features of contemporary society is the multitude of distinct occupations. *The Dictionary of Occupational Titles* lists almost 25,000 different occupations, and other detailed classifications reach similar numbers. This fact alone suggests the difficulty of rational occupational choices. Not only is the number of alternatives beyond the ability of the individual to assess, but many occupations are completely unfamiliar to most people. Many of the day-to-day activities and requirements for success in even relatively familiar occupations are virtually unknown. The image of an occupation is primarily defined for the prospective entrant by those of its activities which are known to the general public, either directly or through media of communication. Training for occupations typically is acquired with respect to "job-families" and not for specific occupations. Vocational preparation and occupational choice are rarely made for a highly specific occupation: few people prepare themselves and make plans for becoming refrigerator repairmen, stock analysts, or public health officers; more likely they think of preparing for mechanical work, business, or medicine.

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Thus, a prospective entrant into the labor force usually is unfamiliar with the many possible fields open to him and has a vague or distorted view of those occupations with which he is relatively familiar. Even when he is undergoing training, his perception of the specific fields to which his training leads him and his possible or probable satisfaction with these fields are unclear. Given this situation, it is exceedingly difficult for the individual to end up in a specific field which is "ideal" for him.

Specialization is often viewed as a functional process for an economic system. However, the functions it fulfills for the individual also should be carefully examined. By providing a series of alternatives for the person who has already chosen a general field, specialization allows him to make secondary choices which can result in closer approximation to an optimum choice. No matter how far removed the primary choice may be from an individual's own interests and abilities, there is the possibility of one or a series of secondary choices which can, at least partially, close this gap.

Most theories of vocational selection stress that it is a continuing process, developing from general, vague and unrealistic choices to specific and realistic ones, Ginzberg,¹ for example, distinguishes three periods: fantasy choices, tentative choices, and realistic choices. Caplow² criticizes the notion that these stages invariably occur, viewing occupational selection more as a continuous process, especially within the educational system. Super³ relates voca-

¹ E. Ginzberg, S. W. Ginsburg, S. Axelrod, and J. L. Herma, *Occupational Choice, An Approach to a General Theory*, New York: Columbia University Press, 1951.

² T. Caplow, *The Sociology of Work*, Minneapolis: University of Minnesota Press, 1954.

³ D. E. Super, *The Psychology of Careers*, New York: Harper Brothers, 1957.

tional choice to psychological stages in the life cycle: the life stages of growth, exploration, establishment, maintenance and decline parallel the work stages of preparatory work, initial work, trial work, stable work and retirement; and both depend on the development and adjustment of the self-concept. Differences in the development of the self-concept may then lead to different kinds of career patterns, and some of the stages may be omitted and modified. The series of choices in the later stages of the process, it is assumed, enable the individual to hold a more realistic appraisal both of the self and of the objective situation. The possible avenues for future choices, however, are limited by previous decisions.

Since much of the interest in this field derives from vocational guidance and because subjects for study are readily available in secondary schools, initial choices of occupation have received considerably more attention than later choices within careers. This paper reports the framework and research design of a study of one example of *secondary choice*, the selection of specialties by physicians and medical students, especially specialization in public health. It includes data collected in three medical schools in the course of designing a national study.

THE CASE OF MEDICINE AND PUBLIC HEALTH

Specialization in medicine is a strategic area in which to study the crystallization of occupational choice. For professions prescribe definite courses of training: pre-professional, general professional, and specialized. Hence the junctures at which decisions must be made, as well as the implications of choices for future training, are formalized. The choice of whether to specialize and in what field must be made by everybody in the profession and has definite consequences for training and work. Moreover, medicine is a highly visible professional career, the physician coming into direct contact with numerous individuals and especially with children, usually under impressive circumstances. Medicine, therefore, is a field which sharply illustrates vocational selection, adjustment of an early ideal to reality, and the formation of professional standards.

A profession like medicine involves a set of prescribed patterns. The individual medical student develops preferences among behavior patterns which correspond to a greater or lesser extent with the professional mold. One of the aims of professional training is to fit the preferences to the prescriptions. But this goal can be met only imperfectly since not all individual differences in needs and values can be molded to one pattern. Secondary choices, then, represent opportunities to achieve greater idiosyncratic satisfactions while still maintaining professional standing.

Different specialties represent different degrees of deviation from the general professional model. Within the medical field it seems that internal medicine, for example, corresponds more closely to the model of a physician than does psychiatry.⁴

In studying the process of secondary choice public health is distinctive in that it represents a particularly deviate specialty. Many features of the occupation of public health physicians are markedly different from the corresponding characteristics of the general field of medicine. An outstanding feature of medical training, a climax of the student's experience, is the clinical work with individual patients. While the core of the "normal" physician's activity is individual diagnosis and treatment, the public health physician, in undertaking community diagnosis and treatment, rarely handles patients on an individual basis, but spends a great amount of his time in administrative and community-relations work. His variety of diagnosis and treatment consists to a great extent, in fact, of administrative acts. Most physicians see themselves, moreover, working as individualistic entrepreneurs—as Hughes has said, the "medical model is a hangover from the outmoded one of the business world."⁵ Contrastingly, public health physicians are salaried, frequently functioning either within strict civil service rules or as political appointees of elected officials.

⁴ H. L. Smith, "Psychiatry in Medicine—Intra or Interprofessional Relationships?" *American Journal of Sociology*, 63 (November, 1957), pp. 285-289.

⁵ E. C. Hughes, "The Making of a Physician: General Statement of Ideas and Problems," *Human Organization*, 14 (1956), pp. 21-23.

Entering public health represents, therefore, a distinct deviation from a normal medical career from the point of view of medical students. But public health physicians must have the same general training as other prospective physicians and expose themselves to the same influences and selection procedures. Here we have a case, then, where preparation for a wider occupation comes in conflict with the choice of a special field. Analysis of this extreme situation is valuable in understanding the general problem of choice of specific occupation.

PRESSURES AGAINST SPECIALIZATION IN PUBLIC HEALTH

The first big hurdle for the prospective physician is admission to medical school. Medical schools generally list certain science courses as a minimum requirement and claim that they do not discriminate in favor of those who were science majors as undergraduates. Nevertheless, the belief is widespread among pre-medical students that a great number of science courses will help admission to medical school. In a recent national survey of medical students 27 per cent of the freshmen reported that they had studied a major in college other than the one they preferred. Two-thirds of these students were influenced to do so by their pre-medical advisers, and almost half thought that this procedure would increase their chances of getting into medical school.⁶ In a panel discussion of this study, an educator comments: "I understand students sometimes say that after acceptance to the medical school they can take various courses they couldn't take before because they didn't dare take them, because their advisers didn't want them to or because taking them might influence acceptance to medical school."⁷ The effect of this attitude is two-fold. The pre-medical student is discouraged from studying in the humanities and social sciences and is therefore less likely to become interested in those social

⁶ H. H. Gee, "The Student's View of the Admissions Process," in *The Appraisal of Applicants to Medical Schools, Journal of Medical Education*, October, 1957, Part II, pp. 140-152.

⁷ Norman F. Witt, "Needs for Liaison With Undergraduate Colleges," *ibid.*, p. 163. Cf. A. W. Schmidt, "Medicine and the Liberal Arts," *Journal of Medical Education*, 32 (1957), pp. 255-262.

and cultural aspects of health and disease which are the concern of public health. On the other hand, the undergraduate student who is interested in the latter problems is less likely to take pre-medical courses and to apply to medical schools and probably, if he does so, is less likely to be admitted.

If the student entering medical school is thus unlikely to be interested in the problems of public health, his instruction in most medical schools will do little to stimulate such an interest. The subject matter which the medical student must absorb during his four years of study is considerable, to which is added his training in the art of clinical practice. Medical schools are understandably reluctant to increase the student's load with courses which seem only vaguely related to the "primary" topics. Of the eighty-three medical schools, twenty-nine have no independent departments of public health or (what is not quite the same) of preventive medicine. And even those existing courses in these fields are often short seminars with time begrudgingly from the major clinical or pre-clinical areas.

Medical schools, of course, teach more than the contents of the formal curriculum: they are also the place where the future physician absorbs the values of the professional group. Among other matters, he learns what kind of work different kinds of physicians do, what standards are used for evaluating different careers, and how a physician should act toward laymen. Public health tends to be seen in these surroundings as a marginal field. The limited instruction in this subject gives students little information about the specialty and also leaves them with the impression that public health is peripheral as a medical profession.

The place of the physician in a public health setting necessitates a good deal of give-and-take with members of other professions as well as with numerous official and non-official community agencies. The public health physician does not have the unquestioned authority physicians tend to possess in the hospital or the air of omniscience which they assume at times toward patients. On many issues concerning the relation of the physician to society, such as the organization of medical care, the public health profession generally espouses different

views from those of the spokesmen of organized medicine. These are matters relatively foreign to medical training.

Finally, the informal interaction among students, with faculty members, and with physicians affiliated with the teaching hospital influences the student's future career. Informal guidance and aid by older members of the profession usually begin in medical school; influential members of the faculty can encourage students toward different careers through offering or refusing help. Oswald Hall has shown how much the success of the physician depends on the relationship which he has been able to establish with other members of the profession, and how the image which the young physician has of himself can determine his future.⁸ With respect to public health, not only is little time generally given to the subject within the medical curriculum, but possibly teachers of preventive medicine are primarily oriented to the context of private practice. In any event, there are few public health role models in medical schools, and those available are inconsistent with the numerous clinical models of other fields of medicine.

THE BASES FOR SECONDARY CHOICE

The pressures within the profession, then, against the choice of public health are very strong. Commitment to a career of medicine as the primary choice, therefore, seems almost to preclude specialization in public health (the secondary choice). Yet the latter career is selected by a minority, a fact begging explanation.

Secondary choices may function to correct discrepancies between preferred modes of activity and the professional model. Physicians who enter public health presumably have original dispositions which are markedly inconsistent with this model. They must also be able to resist strong pressures. In the following sections data are presented which illustrate the origin and maintenance of this deviant pattern. The data are taken from the medical school setting, revealing clearly the impact of the medical model on the indi-

vidual, with his unique dispositions, and the degree to which a molding process occurs.

Personality. In accordance with this formulation, we may expect differences in personality traits between physicians in public health and other physicians. Here two areas seem to be especially important: the contrast between individual preferences and organizational endeavor; and the nature of social objects of influence, that is, whether the individual feels more comfortable dealing with people as individuals or in groups. These two areas are closely related to the differences between public health and other medical specialties. The work of the physician generally is with individual patients; he is trained to assume authority and to inspire confidence in an essentially two-person situation. Correspondingly, he can work alone, or in a loose association with other physicians. The public health physician is concerned with the community as a whole; he can be most effective as a group worker. Not an individual practitioner, he must function within some organization, and, ideally, he should have traits that maximize such activity.

Comparisons were made of test scores of thirty-two students not intending to go into public health and of seven physicians who were students in a school of public health at an Eastern University. On the Edwards Personal Preference Scale, the two groups showed significant differences (five percent level) in that the medical students had higher scores for achievement and aggression needs, the public health students for deference, abasement and nurturance needs. On the Allport-Vernon-Lindzey Study of Values, the two groups differed significantly on four scales: the medical students scored higher on theoretical and aesthetic values, the public health students on social and religious values. As expected, the needs and values of the medical students refer to individual achievement and enjoyment, while those of the public health students relate to identification with or subordination to a larger whole. Although these groups are not strictly comparable—the public health students are older and already have professional experience—the differences are consistent with our hypothesis.

Peer Influence. Students may be exposed to differential influences within the medical

⁸ Oswald Hall, "The Stages of a Medical Career," *American Journal of Sociology*, 53 (March, 1948), pp. 327-333, and "Types of Medical Careers," *American Journal of Sociology*, 55 (November, 1949), pp. 243-253.

school. Sociometric study can trace the influence pattern among the students and between students and faculty. Other studies of influence suggest the hypothesis that a group standard concerning the preference of specialties develops in medical school, and that students who are least frequently chosen by their peers on sociometric tests because of limited prestige or personal attraction are more likely to deviate from the group standard.

A study was made of four classes of a Southern medical school.⁹ From the freshman to the senior class, students seem increasingly to move in the direction of what they conceive the group standard to be. They were asked to rank fifteen fields of medicine, first, on the basis of their own interests and, second, according to their perception of the interests of most medical students. In each of the fifteen fields the average choice of the seniors was different from that of the freshmen. The trend from the freshman to the senior year generally was in the same direction as the difference between the freshmen's perception of the interests of most medical students, and their own choices. The data upon which Table 1 is based show this pattern in twelve of the specialties, if ties are eliminated. For example, freshmen give surgery an average rank of 6.8 with respect to personal interest. But their conception of how most medical students would rank surgery (the group standard) is more favorable and averages 2.5. Seniors give surgery an average rank of 5.4 in terms of personal interest, a change upward reflecting, no doubt, the more favorable group standard.¹⁰ Public health, contrastingly, illustrates a trend in the opposite direction. This field ranks 11.5 (a low rating) in terms of personal interest among freshmen and 12.8 (an even lower rating) according to their view of how most medical students would rank this field. Seniors give it an average rank of 12.7.

Evidence of the influence of student relationships on the rank order of choices can

⁹ The students studied numbered 253 of a school enrollment of approximately 300. All students were requested but not required to attend an assembly at which questionnaires were completed.

¹⁰ This change from freshmen to seniors, however, does not occur in gradual intermediate steps from year to year.

TABLE 1. FRESHMAN AND SENIOR PREFERENCES FOR SPECIALTIES AND FRESHMEN'S PERCEPTION OF GROUP PREFERENCE

Direction of Difference Between Freshmen and Senior Choices

Seniors (Compared with Freshman)	More Favorable	Less Favorable
Differences between perceived group choice and own choice (Freshmen)		
Group choice: More favorable	5	1
Less favorable	0	7
Same	2	0

(Entries are number of specialties rated)

be found in an analysis of sociometric patterns. As direct questions about influence of other students typically bring denials, an indirect approach was used.

If students arrive at consensus through mutual discussion and persuasion, it is likely that those who have more contacts with others would learn the generally accepted order of preference earlier than students with fewer contacts. The degree of contact was measured by three interrelated questions in a sociometric questionnaire: "Whom do you like best?" "To whom do you talk most about your future?" and, "To whom would you go for advice on a personal problem?" The four classes were divided into three approximately corresponding popularity groups: high (three or more choices), medium (one or two choices), and low (no choices). In the resulting twelve groups (three popularity groups in four classes), agreement on preferred specialties was computed by Kendall's W, the coefficient of concordance. Table 2 presents the results.

There is little difference from class to

TABLE 2. AGREEMENT ON PREFERENCE FOR SPECIALTIES OF DIFFERENT POPULARITY GROUPS *

(Coefficients of Concordance)

	High Popularity	Medium Popularity	Low Popularity
	W	W	W
Freshmen	.45	.44	.29
Sophomores	.52	.43	.36
Juniors	.40	.51	.62
Seniors	.56	.67	.54

* N of raters ranges between 12 and 35 per group. N of specialties rated is 15 in every case.

class in agreement about the highly popular group of specialties; this consensus seems to be established in the freshmen year. On the other hand, in the "low" group, the coefficient of concordance increases during the first three years. Students who have more contacts and are therefore presumably exposed to more peer-group influence reach the common standard of agreement more quickly than the isolates.

Additional evidence is provided by inspecting the relation between the average ranks of interest for specialties of each group. The average rank order correlation of interests between the high and low popularity groups converges from the freshman to the senior year. The correlation coefficient between the two groups is .72 for freshmen, increases for sophomores and juniors, and reaches .95, close to perfect agreement, for seniors. Whatever their popularity, students show very similar preferences in the senior year.

Faculty Influence. Faculty influence on student choice of field may be highly effective. Kendall and Selvin¹¹ suggest, for example, that low-ranking students indicating a preference for a specialized internship are discouraged by staff members from applying for one. Conversely, high-ranking students expressing interest in such specialized training seem to receive encouragement and support from the staff.

Students' choices of faculty members provide information on this influence pattern. They were asked a series of sociometric questions about members of the faculty they liked best in general, as teachers, as doctors, and as sources of advice on personal, scholastic, and career problems.¹² Half of all of the responses to these questions refer to only eight individuals of the total two hundred faculty members. But the patterns vary ac-

cording to year in medical school, each class tending to concentrate on different individuals. There is a trend, moreover, away from choices of the preclinical science faculty and toward choices of the clinical faculty in succeeding classes, culminating in the senior class. Thus the clinician has the greater influence on the graduating physician.

The modal direction of this influence is indicated by inspecting the responses of the senior class. The two individuals most frequently chosen account for 50 per cent of choices on the question, "Who is the best doctor?" and 45 per cent on "Whose advice would you ask for first on which field of medicine to enter?" Responses to other questions are less concentrated, ranging from 35 per cent ("Who is the best teacher?" and "To whom would you take your scholastic problems?") to 20 per cent ("Whom do you like best?"). On all seven questions, the two faculty members most frequently named were members of the Departments of Internal Medicine, Obstetrics, or Pediatrics.

The emergent pattern, then, seems to be that a small proportion of the staff are the sociometric stars in diverse areas, that a few from among these become more influential in student selection of specialties as graduation approaches, and that these individuals, representing a limited number of clinical fields, provide medical role models for the students.

Perception and Valuation. Personality and social influences have important consequences for secondary choice. But what is it that medical students like or dislike about an occupation? What are their images of the specialized fields?

Students in the Southern medical school were asked to rate fifteen medical activities according to their appeal to them on a scale ranging from "Very Much" to "Not At All." The examples were designed so as to represent five types of activity: diagnosis, therapy, ancillary work, supervision, and community relations.¹³ The five types are shown in the

¹¹ P. L. Kendall and H. C. Selvin, "Tendencies Toward Specialization in Medical Training," in R. K. Merton, G. Reader, and P. L. Kendall (editors), *The Student Physician*, Cambridge: Harvard University Press, 1957, pp. 153-174.

¹² The items were: (1) Whom do you like best? (2) Who is the best teacher? (3) To whom would you take your scholastic problems? (4) To whom would you take your personal problems? (5) Whose advice would you ask first on which field of medicine to enter? (6) To whom have you talked about your future? (7) Who is the best doctor?

¹³ These five types of activities were based on a study by E. M. Cohart and W. R. Willard, "A Time Study Method for Public Health — The Yale Study," *Public Health Reports*, 70 (1955) pp. 570-576; and "Functional Distribution of Working Time in Five County Health Departments," *ibid.*, pp. 713-719.

context of different specialties, for example: "Determining the source of an outbreak of food poisoning" (diagnosis, public health); "Performing an appendectomy" (therapy, surgery); "Giving a talk to the Parent-Teachers Association on what the family physician wants to know when you phone him" (community relations, general practice). In this way, the relative influence of the kind of activity or the specialty itself in student preferences may be estimated.

Table 3 shows the results of the analysis of variance of these ratings. The main effects of both activities and type of specialty are highly significant. There is a definite order of preference for activities among students: diagnosis, therapy, supervision, ancillary work, and community relations. But each activity is definitely less liked when associated with public health than with other fields of medicine. For instance, the average rating for diagnosis in public health is only as favorable as the rating for supervision in general practice. The interaction effects, although significant, are very small in comparison with the two main effects (the main

effects are significant even in comparison with the interaction effects). The interaction between specialty and activity is the result of certain upsets of general pattern, probably a function of the particular example used in the question, while the change of preference for activities over the years of school produced no consistent trends. Public health, therefore, is handicapped in the eyes of the students on two grounds: activities which characterize it are rated low in general and any activity is rated lower if it is connected with public health.

Another approach to perception of the specialties involved an open-ended question on what students would like most and least about working in eight fields of medicine. Table 4, which summarizes the data obtained in this way includes only the five most frequently mentioned categories for each specialty.

These results indicate a fairly high degree of uniformity in the perception of the specialties. For one thing, there are very few instances in which the same field is mentioned as "liked most" and "liked least" on a given

TABLE 3. PREFERENCE RATING FOR MEDICAL ACTIVITIES ASSOCIATED WITH DIFFERENT SPECIALTIES

Activities:	Surgery	(A) Mean Ratings		
		General Practice	Public Health	Total
Diagnosis	1.04	1.54	.52	1.03
Treatment	.78	.73	-.12	.46
Supervision	.50	.59	-.02	.36
Ancillary	.61	.43	-.75	-.10
Community relations	-.30	.20	-.28	-.13
Total	.53	.70	-.13	
N = 257				

Scores are computed on a five-point scale, ranging from +2 ("appeals very much") to -2 ("not at all").

(B) Analysis of Variance

Source:	d.f.	Sum of Squares	Mean Squares	F
Activity	4	591.241	147.81	204.*
Specialty	2	488.097	244.048	337.*
Class in school	3	3.814	1.271	1.76
Act. x Specialty	8	150.368	18.796	26.0*
Act. x Class	12	42.100	3.51	4.85*
Class x Specialty	6	6.378	1.063	1.47
Act. x Spec. x class	24	18.637	.776	1.07
"Error"	3796	2750.165	.724	

* P<.01.

TABLE 4. FIVE MOST DESIRABLE AND UNDESIRABLE ASPECTS OF FIELDS WITHIN THE MEDICAL PROFESSION *

	Public Health	Dermatology	General Practice	Internal Medicine	Pathology	Psychiatry	Surgery	Teaching
Degree to which there are close relationships with patients	U	—	D	D	U	D	U	U
Variety of activities	D, U	D, U	D, U	D, U	D, U	—	D, U	U
Hours of Work	D	D	U	U	D	D	—	D
The particular kind of work involved	U	U	—	D	U	D	D	D
Problems necessitating exacting thought	D	D	D	D	D	—	—	—
Amount of certainty of effect	—	U	U	U	—	U	D	—
Availability of opportunities for helping people	D	U	D	—	—	D	D	D
Type of patients	—	U	U	U	—	U	—	—
Degree of opportunity for learning	—	—	—	D	D	D	—	D
Income	—	D	—	—	—	—	—	U
Physical Aspects of the work	—	—	U**	—	U**	—	U**	—
Degree of independence possible	U	—	—	—	—	—	—	U
Degree to which there are close relationships with colleagues, community people	D	—	D	—	—	—	—	—
Degree of research involved in the work	—	—	—	—	D	—	—	D, U
Consequences of failure	—	D	—	—	—	—	U	—
Abilities involved	U	—	—	—	—	U	D, U	—
Type of colleague	—	—	—	—	—	U	—	—
Emotional aspects of the work	—	—	—	—	—	U	—	—
Difficulties of attainment	—	—	—	U	—	—	—	—
Danger of contagion	—	—	—	—	U	—	—	—

* D indicates the aspect perceived as desirable, while U indicates it perceived as undesirable.

** In the case of pathology, physical inactivity constituted an undesirable aspect of the field; with respect to general practice and surgery, physical strain was perceived as undesirable.

factor by different respondents.¹⁴ Another indication of uniformity is that each specialty is accorded a relatively unique combination of factors. Although some factors are mentioned for a number of specialties, their frequencies of response usually vary.

The relationship of a given uniformity of affect to the socialization process within the

medical school may be inferred from Table 5, which presents an example of the responses of different classes. The three fields listed are those for which "lack of close relationships with patients" appears in Table 4 as a factor which is liked least. The increasing proportion of responses, as well as the absolute degree of uniformity in perceiving the specialties, suggests the development of norms with respect to desirable and undesirable aspects of the different fields.

¹⁴ The chief exception here is the category "variety of activities," which is perhaps a relatively inclusive and indefinite one.

TABLE 5. "LACK OF CLOSE RELATIONSHIPS WITH PATIENTS" AS THE FACTOR LIKED LEAST IN THREE FIELDS

	Freshmen	Sophomores	Juniors	Seniors
	%	%	%	%
Pathology	37 (78)*	45 (78)	57 (60)	59 (54)
Public health	14 (85)	29 (83)	43 (67)	41 (56)
Teaching	22 (80)	32 (77)	29 (69)	36 (53)

* Numbers in parentheses show the total number of responses in each class.

Commitment to Career. Our final approach concerns the whole career line. We have discussed and illustrated the hypothesis that public health is a deviant choice often resulting from incomplete socialization within the medical school. It is likely, moreover, that the selection of public health in many cases is made after leaving medical school. One study of local and state health departments, for example, reveals a typical pattern in which the physician enters this field some years after his medical schooling and after he has worked in one or more positions other than public health.¹⁵ Among the approximately 300 students from three medical schools about whom we have information, not one gave public health as his first choice. A small percentage of these physicians, however, probably will go into the public health field. If this is the case, physicians entering public health change their career plans more frequently than those who do not.

Some supporting data for this hypothesis are to be found in a recent study.¹⁶ Stiles and Watson asked public health students to rate 26 factors according to their importance in choice of a profession. They rated

more highly than did students in other health professions (dentistry, medicine, pharmacy, nursing) the following factors: an agreeable opportunity, close relation to the favorite occupation, influence of a teacher, environmental influence, and military experience. Although a large proportion of public health students are not physicians, the fact that all of these reasons refer to change of plans is consistent with our reasoning. Comparison of the career patterns of diplomates in preventive medicine and public health with those in internal medicine, surgery, pathology, and psychiatry, shows additional support for this hypothesis (Table 6). Examination of a sample of the listings in the *Directory of Medical Specialists*,¹⁷ indicates that specialists in preventive medicine are more likely than others to list experience in different medical fields. Even qualification as a specialist in a different field, which implies considerable experience in this specialty and thus is infrequent, occurs among specialists in preventive medicine more often than elsewhere.

These findings concerning careers, although fragmentary, point up again the marginal position of the public health physician. The physician who is less committed to a specific career and receptive to accidental influences probably will eventually select a deviant specialty.

¹⁵ E. M. Cohart and W. R. Willard, "Experience of Public Health Workers," *Public Health Reports*, 70 (1955), pp. 1116-1124.

¹⁶ W. W. Stiles and Lois C. Watson, "Motivation of Persons Electing Public Health as a Career," *American Journal of Public Health*, 45 (1955), pp. 1563-1568.

¹⁷ *Directory of Medical Specialists*, Vol. VIII, Chicago: Marquis—Who's Who, 1957.

TABLE 6. EXPERIENCE IN OUTSIDE SPECIALTIES OF PHYSICIANS IN DIFFERENT SPECIALTIES

	Preventive Medicine	Internal Medicine	Surgery	Pathology	Psychiatry
Per cent listing experience in other fields of medicine	29.0	26.4	24.0	5.6	17.5
Per cent qualified by a different board	7.0	1.6	4.0	.8	1.9
Number	(500)	(125)	(125)	(125)	(103)

ETHNIC GROUPS AND THE PRACTICE OF MEDICINE *

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MEDICAL practice in a city may be viewed as a system resulting from competition between physicians for the more desirable practices and the efforts of patients to obtain the best physicians available. If physicians were guided solely by the financial position of patients and if patients could select their physicians on the basis of their relative technical competence, then a simple economic system would result.

Since patients are generally unable to judge practitioners in terms of technical competence, extraneous factors actually determine their choice.¹ Accordingly, the competitive position of a doctor is greatly influenced by his ability to fulfill expectations which are not medically relevant. This implies that the performance of the physician role—despite its high status, relatively specific function, and limitation to persons properly trained and legally sanctioned—is greatly affected by the doctor's other status positions. In effect, this means that other statuses are factors which affect preferences.

The importance of ethnic group membership as a factor in medical practice has been stressed in several studies.² This paper considers the effect of ethnic origins of physicians on the practice of medicine. The spatial distribution and degree of specialization of Anglo-Saxon, Irish, Italian, Jewish, and

Polish physicians in Chicago were studied. The study assumes that the patient is highly concerned with his choice of physician, but does not select him on purely medical grounds. As a consequence, certain non-technical criteria are assumed to influence this choice.³

HYPOTHESES

In observing the significance of such "auxiliary status characteristics," Hughes has suggested that the resolution of these status problems frequently involves some form of social segregation of clients or services.⁴ If the status of a given ethnic origin is not disadvantageous among members of the ethnic group itself, then one effect would be the tendency for physicians to have clients who are members of their own ethnic group. Such restriction of practice suggests the first hypothesis: Physicians of a given ethnic group locate their offices in accordance with the residential distribution of their ethnic group.

Since there are ethnic groups with a disproportionately large number of physicians, some doctors cannot resolve their status conflict by practicing amongst their own ethnic clientele. For example, Jews comprise roughly seven per cent of Chicago's population but make up about 30 per cent of Chicago's physicians.⁵ For these physicians to compete successfully for out-group patients with physicians who are themselves members of the out-group, it is necessary to introduce additional auxiliary statuses which, unlike ethnic status, tend to support and strengthen the medical role.

* The author is pleased to acknowledge the helpful advice of James S. Coleman and Otis Dudley Duncan.

¹ Oswald Hall, "The Informal Organization of the Medical Profession," *Canadian Journal of Economics and Political Science*, 12 (1946), p. 30; Talcott Parsons, *The Social System*, Glencoe, Ill.: The Free Press, 1951, Chapter 10.

² Oswald Hall, "The Informal Organization of Medical Practice in an American City," Ph.D. dissertation, University of Chicago, 1944; David N. Solomon, "Career Contingencies of Chicago Physicians," Ph.D. dissertation, University of Chicago, 1952; Josephine J. Williams, "Patients and Prejudice: Lay Attitudes toward Women Physicians," *American Journal of Sociology*, 51 (January, 1946), pp. 283-287.

³ Williams, *op. cit.*

⁴ Everett C. Hughes, "Dilemmas and Contradictions of Status," *American Journal of Sociology*, 50 (March, 1945), pp. 353-359.

⁵ Jewish population estimate from Beverly Duncan, "Estimated Jewish Population of Chicago and Selected Characteristics, 1951" (Unpublished study of the Chicago Community Inventory); estimate of Jewish physicians from Solomon, *op. cit.*, p. 17.

If "ambiguity of role qualifications is conducive to sociocentrism,"⁶ the groups overrepresented in medicine may be expected to need and use devices which reinforce their medical status. Specialization, as opposed to general practice, serves such a function since specialists have higher prestige and there is a more formal relationship between doctor and patient. Similarly, location of an office in the high status central business district (the Loop), provides an extraneous element which reinforces the role of physician. If specialization and Loop location involve higher prestige than general practice in a local neighborhood, then physicians competing for out-group patients are more apt to use these devices for status reinforcement. Hence the second hypothesis: Physicians of ethnic groups overrepresented in medicine are more likely to practice in the Loop and to specialize than are physicians who are members of ethnic groups underrepresented in medicine.

METHODS AND DATA

A 10 per cent random sample was taken from the 7,477 physicians practicing or living, or both, in Chicago in 1950, according to the *American Medical Directory*.⁷ The medical data were therefore comparable, chronologically, with 1950 census data for Chicago. Physicians listed as interns, residents, medical officers in the armed forces, practicing solely in hospitals, retired, or as not in practice were removed from the sample. The data for the remaining 576 physicians were used to ascertain the location of homes and offices, medical school attended, and medical specialties of Chicago physicians.

A second sampling procedure was employed for the groups of physicians under consideration. From a list of surnames associated with each of the five ethnic groups, physicians were selected from the *Directory* until at least 100 doctors practicing outside of the central business district were obtained for each group. This method places emphasis on those physicians whose sur-

names carry a strong ethnic connotation and are therefore most readily identified and confronted with the hypothesized status problem.⁸ Since this technique does not yield estimates of each group's representation in medicine, other methods were used for this problem and are described below. While this is not an altogether valid means for determining the ethnic membership of doctors, errors made in placing physicians with an incorrect group tend, in general, to work against the hypotheses under study, that is, to reduce the chances of finding differences between ethnic groups. The omission of physicians who have changed their surnames, possibly in an effort to conceal their ethnic identity, is not an important handicap to the research because such doctors are not affected significantly by the hypothesized status problems.

Since surname was the sole criterion for selection of members of the five ethnic groups, such dependent variables as office location have not affected the placement of doctors into the ethnic categories. Although the independent variables in this study are the ethnic groups associated with physicians' surnames—not actual ethnic membership—it seems reasonable to assume that differences between physicians grouped according to surname are functions of the ethnic group associated with the surname. Had the technique for distinguishing doctors according to name failed as an indicator of ethnic membership, one would expect to find no differences between the five groups, or between any one of the groups and the random sample of all Chicago physicians.

The analysis of the distribution of the ethnic populations within the city of Chicago was based on the 1950 census data available for the first generation of each group.⁹ The 75 "community areas" of Chicago were used to distribute the physicians and ethnic populations into spatial units.¹⁰ The rela-

⁶ Jackson Toby, "Universalistic and Particularistic Factors in Role Assignment," *American Sociological Review*, 18 (April, 1953), p. 141.

⁷ Frank V. Cargill, editor, 18th Edition, Chicago: American Medical Association, 1950.

⁸ Use of a list of Negro physicians in Chicago in 1950, made available to the author by Dietrich C. Reitzes, permitted the elimination of Negroes from the ethnic groupings.

⁹ Data for the distribution of Jewish population in Chicago were drawn from Duncan, *op. cit.*, pp. 40-42.

¹⁰ For a brief summary of the history and nature of the 75 Community Areas of Chicago,

ship between ethnic identity of physicians and ethnic membership of clientele involves the assumption that a physician's practice comes primarily from the community area in which his office is located. Further, it was assumed that the spatial distribution of the foreign born is an adequate indicator of the distribution of later generations of the ethnic population.¹¹ These assumptions, while gross, appear adequate for the investigation.

An index of dissimilarity was used to provide a simple method for comparing the spatial distribution of one population with another. This index indicates the percentage of a population which would have to redistribute itself in order to have the same proportional distribution as another population. Thus, complete dissimilarity in the spatial distribution of two groups would yield the maximum index value, 100; complete similarity would yield the minimum index value, 0.¹²

FINDINGS

Spatial Distribution of Ethnic Physicians' Offices. One of the implications drawn from the analysis of auxiliary status characteristics is that ethnic doctors practice in areas where members of their own ethnic group live. This involves the assumption that the ethnic background of a physician is a handicap in dealing with patients who are not members of the same ethnic group, but is not a handicap, or is a lesser one, in practicing among patients who are members of the physician's group. Indexes of dissimilarity were obtained to determine the extent to

see Philip M. Hauser and Evelyn M. Kitagawa, editors, *Local Community Fact Book for Chicago*, Chicago: Chicago Community Inventory, University of Chicago, 1953, p. xi.

¹¹ For a comparison of the residential distributions of first and second generations in Chicago in 1930, see Otis Dudley Duncan and Stanley Lieberson, "Ethnic Segregation and Assimilation," to be published in the *American Journal of Sociology*.

¹² For fuller discussions, see Otis Dudley Duncan and Beverly Duncan, "A Methodological Analysis of Segregation Indexes," *American Sociological Review*, 20 (April, 1955), pp. 210-217; Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, 60 (March, 1955), pp. 493-503.

which the office locations of groups of physicians approximate the residential distribution of selected ethnic populations.

The rows in Table 1 indicate that each group of physicians is closer to its own ethnic population than to any other population. The random sample of all Chicago physicians is closer to the total city population than to any of the ethnic populations. Similarly, the columns of Table 1, with two minor exceptions (Anglo-Saxon population versus Jewish and random sample physicians; and Irish population versus random sample physicians) demonstrate that each group's residential distribution is closest to the office distribution of the group's physicians.

The use of indexes of dissimilarity presents certain difficulties in comparing the distribution of one group's physicians with the residential distribution of another population. The problem in its general form is that as the spatial distribution of group A approaches the spatial distribution of group B, there is a similarity in distribution between A's physicians and B's population, even if A's physicians are simply following the distribution of group A. Therefore an additional procedure was employed. All community areas were separated into those with a minimum of 1.3 per cent of the total city population of a given ethnic group, and those with less than this arbitrary minimum. When this technique was used for each of the five groups a number of community areas were found to have such minimum populations for several groups. At least 75 per cent of each ethnic group's total city population was found to reside in community areas containing the minimum of 1.3 per cent of the ethnic group's total population.

Table 2 (part A) demonstrates the important effect of an ethnic population base upon the location of the group's physicians. If doctors located themselves without being influenced by ethnic factors, the ratio would be 1.0, that is, the proportion of an ethnic group's neighborhood physicians practicing in these areas would be equal to the proportion of the total city population in these community areas. With one exception, physicians show ratios above 1.0 in the first part of Table 2.

Part B of Table 2 shows the extent to

TABLE 1. INDEXES OF DISSIMILARITY BETWEEN OFFICE LOCATIONS¹ OF RANDOM SAMPLE OR SELECTED GROUPS OF ETHNIC PHYSICIANS PRACTICING OUTSIDE THE LOOP AND RESIDENTIAL LOCATIONS OF SELECTED POPULATIONS, CHICAGO, 1950

Physicians' Classification	Anglo-Saxon ²	Selected Ethnic Populations					City Population
		Irish	Italian	Jewish	Polish		
Anglo-Saxon (N 117)	38.8	45.5	57.1	51.8	73.1	43.3	
Irish (N 110)	43.0	40.2	59.7	60.2	73.4	49.6	
Italian (N 116)	53.6	52.4	30.6	70.0	69.9	50.1	
Jewish (N 102)	34.1	49.3	55.1	31.4	64.4	44.7	
Polish (N 102)	72.2	76.1	65.3	76.2	29.9	61.4	
Random Sample (N 438)	27.0	38.6	45.1	49.2	55.6	22.4	

¹ For physicians having more than one office outside of the Loop, each office given weight of one.

² Persons born in England and Wales.

Note. Each row of indexes compares the office distribution of physicians of a given group with the residential distributions of six populations. Each column of indexes compares a given population's residential distribution with the office distributions of each of the six groups of physicians.

which physicians practice in areas where less than 1.3 per cent of their ethnic group's total city population reside, but where another group has at least 1.3 per cent of its total population in the city. Here all of the ratios fall below 1.0. This suggests that physicians are much less likely to prac-

tice in out-group areas if their own ethnic population is not present to form a base for the doctor's clientele.

Spatial Distribution of Ethnic Physicians' Homes. The similarity in office locations and residential distributions of a given ethnic group was noted above. Can this association be explained as a function of the factors which produce the ethnic group's residential segregation? The home location of physicians is a situation in which the interplay between physician and patient is not relevant. Consequently, if the segregation of physicians along ethnic lines is not due simply to the segregation of all members of their ethnic group but is actually intensified by the vital and ambiguous relation between doctor and patient, then ethnic membership would be expected to play less of a role in the residential location of physicians than in their office locations.

The data for Irish, Italian and Polish physicians indicate that they are influenced along ethnic lines in the distribution of their offices more than in the location of their homes (Table 3, rows 1 and 3), although ethnic membership still appears to be an important factor in influencing residential location (rows 3 and 4). While Jewish and Anglo-Saxon physicians are hardly closer to their ethnic population professionally than residentially, this is probably due to the location of their ethnic populations in the more desirable sections of the city. Supporting this view are the tendencies of Irish, Italian, and Polish physicians to show closer distributions by home than

TABLE 2. RATIO OF THE PER CENT OF PHYSICIANS PRACTICING IN SELECTED COMMUNITY AREAS TO THE PER CENT OF THE TOTAL POPULATION LIVING IN THESE AREAS, CHICAGO, 1950

Physicians	Part A				
	Areas with Minimum of 1.3 Per Cent of the Physician's Total Ethnic Population and Population of—				
	Anglo-Saxon	Irish	Italian	Jewish	Polish
Anglo-Saxon	...	1.5	1.2	1.2	0.4
Irish	1.7	...	1.2	1.7	1.1
Italian	2.0	2.5	...	1.4	1.4
Jewish	1.7	1.7	2.0	...	1.6
Polish	2.1	3.8	2.3	2.1	...

Physicians	Part B				
	Areas with Less than 1.3 Per cent of the Physician's Total Ethnic Population but with Minimum of 1.3 Per cent of Total Population of—				
	Anglo-Saxon	Irish	Italian	Jewish	Polish
Anglo-Saxon	...	0.8	0.1	0.0	0.4
Irish	0.3	...	0.4	0.2	0.4
Italian	0.5	0.5	...	0.5	0.4
Jewish	0.2	0.5	0.4	...	0.1
Polish	0.2	0.2	0.2	0.2	...

TABLE 3. INDEXES OF DISSIMILARITY BETWEEN OFFICES OR RESIDENCES OF PHYSICIANS AND RESIDENTIAL LOCATIONS OF SELECTED POPULATIONS, CHICAGO, 1950

Distributions Compared *	Physicians' Ethnic Classification					Random Sample
	Anglo-Saxon	Irish	Italian	Jewish	Polish	
Office Locations vs. Distribution of:						
1. Physicians' Ethnic Population	38.8	40.2	30.6	31.4	29.9
2. City Population	43.3	49.6	50.1	44.7	61.4	22.4
Home Location vs. Distribution of:						
3. Physicians' Ethnic Population	39.8	45.0	42.7	31.3	40.9
4. City Population	45.7	56.5	50.4	54.3	57.9	31.4

* Note. Offices of physicians compared with residences of: their ethnic population (Row 1); the total city population (Row 2). Residences of physicians compared with residences of: their ethnic population (Row 3); the total city population (Row 4).

by office with the residential distributions of Anglo-Saxon and Jewish populations. Moreover, the random sample indicates that Chicago physicians are more likely to select Anglo-Saxon and Jewish residential areas in locating homes than in locating offices, although the sample's homes are more segregated from the total city population than the offices. Actually, the home indexes of dissimilarity for physicians against their respective ethnic population would be higher for all groups if physicians who practice in the city proper, but live in the suburbs, had been included in the residential calculations or excluded from the office calculations.

In locating their practices, the medical profession, as a whole, (as represented by the random sample), shows a closer approximation to the residential distribution of the city population than does any single ethnic component of the medical profession (Table 3, row 2). The physicians' offices of each ethnic group show a closer approximation to the group's own ethnic population than to the total city population (compare rows 1 and 2), while the random sample of all Chicago physicians is marked by an office distribution closer to the residential distribution of the total city population than to that of any of the ethnic populations under consideration (Table 1).

The index of dissimilarity of 22.4 between the random sample of physicians and the total city population means that nearly 80 per cent of all medical locations in the city, but outside the Loop, are distributed proportionately to the spatial distribution of Chicago's population. Each of the five ethnic

components under consideration, however, appears to be otherwise influenced in the selection of office locations. This suggests that the distribution of the medical profession, in approximating the city population pattern, takes place as the offices of each ethnic segment are segregated along ethnic lines. The physicians of each ethnic group specialize in serving their fellow-ethnics, although the net effect for all physicians is a closer approximation to the city population than that produced by the distribution of any single group of physicians considered in this study. Since Chicago physicians include members of ethnic groups not considered here, it is assumed in the above discussion that other ethnic groups have similar relations with their ethnic populations.

Ethnic Representation. There is an association, then, between physicians and patients along ethnic lines. But what about physicians who are members of ethnic groups having an excess of doctors, that is, more doctors than the ethnic population needs?

It was indicated above that, in this sense, Jews are overrepresented in medicine in Chicago. Since there are no similar data on the number of physicians in Chicago by ethnic origin, it was necessary to estimate the representation of the other ethnic groups. A set of rates was computed for each ethnic population: foreign born males, foreign born females, native males of foreign or mixed parentage, native females of foreign or mixed parentage. These rates express the subgroup's number of physicians and dentists in the United States as a per cent of its pro-

TABLE 4. DISTRIBUTION OF PHYSICIANS ACCORDING TO MEDICAL SCHOOL ATTENDED AND PER CENT OF GRADUATES WHO ARE FULL-TIME SPECIALISTS IN RANDOM SAMPLE OF CHICAGO PHYSICIANS, 1950

Medical School Attended	Ethnic Group					Random Sample	Per Cent of Random Sample Who Specialize
	Anglo-Saxon	Irish	Italian	Jewish	Polish		
Number of Physicians ¹	164	135	126	155	108	575
University of Chicago and Rush	21.3	8.8	5.6	17.4	6.4	13.2	67.1
Northwestern	21.3	13.4	2.4	3.8	3.7	12.1	47.1
University of Illinois	14.1	15.5	20.6	38.1	17.6	24.6	40.4
Chicago Medical School	0.6	3.0	1.6	1.9	2.8	2.6	26.7
Stritch School of Medicine of Loyola	10.3	43.0	34.1	7.1	38.9	10.9	25.4
Medical School Purchased by Loyola in 1917	4.9	2.9	1.6	3.9	4.6	4.7	18.5
Schools Not Recognized by A.M.A. ²	6.1	2.2	19.8	12.3	14.8	7.3	14.3
Other Illinois ³	5.5	2.3	2.4	1.3	3.7	5.2	16.7
Other Recognized Schools in U. S.	11.6	6.6	2.4	6.4	2.8	12.4	50.7
Recognized Foreign	4.3	2.3	9.5	7.8	4.7	7.0	45.0
All Schools	100.0	100.0	100.0	100.0	100.0	100.0	40.3

¹ Physicians reporting medical school.² May include foreign and/or American schools.³ Extinct or merged.

fessional, technical, and kindred workers in the nation's labor force in 1950. These rates were applied to the (roughly equivalent) professional, technical, and kindred category in the Chicago Standard Metropolitan Area for each group. Applying national figures, by sex and nativity, to the proportion of physicians in the larger "physician and dentist" category, estimates for each ethnic group were made. Jews and Anglo-Saxons were found to be overrepresented in medicine, especially the former, whereas the remaining three ethnic groups were estimated to be no more than proportionately represented in medicine in Chicago.¹³

Specialization. Specialization has several advantages for physicians in their relations with patients who are members of ethnic groups other than their own. Specialization involves a more specific relationship between doctor and patient. Whereas the specialist is limited in his interaction with a patient to the health problems which are his specialty, the functions of the general practitioner are

less clearly defined.¹⁴ For specialists, relatively rapid patient turnover means that they are less likely to know patients on an informal basis. The general practitioner, moreover, is more likely than the specialist to treat all members of a family. Specialists, when called in through referrals of general practitioners, have had their competence validated, at least implicitly, by a qualified intermediary—the family physician. Therefore patients do not need to use extraneous criteria.

In considering differences in the degree of specialization between ethnic groups, it is necessary to examine their medical school training as a possible alternative explanation of such variations. It can be seen in Table 4 that the physicians in the random sample vary greatly in per cent specialized when classified according to their medical schools. For example, physicians in Chicago who are graduates of Rush and the University of Chicago Medical schools are much more likely to specialize than those who attended The Stritch School of Medicine

¹³ For detailed discussion of the method used, see Stanley Lieberson, "Ethnic Groups and Medicine," unpublished M.A. thesis, University of Chicago, 1958.

¹⁴ See Earl Shepard Johnson, "A Study in the Ecology of the Physician," M.A. thesis, University of Chicago, 1932.

TABLE 5. PER CENT OF PHYSICIANS WHO ARE FULL-TIME SPECIALISTS AND PER CENT PRACTICING IN LOOP, BY ETHNIC MEMBERSHIP, CHICAGO, 1950

Characteristics and Basis of Comparison	Ethnic Group					Random Sample
	Anglo-Saxon	Irish	Italian	Jewish	Polish	
Per Cent Specialized						
Actual	43.3	32.6	19.0	48.4	20.4	40.3
Expected on Basis of Medical School	43.3	35.8	31.3	40.6	30.9	40.3
Actual Minus Expected	0.0	-3.2	-12.3	7.8	-10.5
Per Cent Offices in Loop						
All Physicians	30.4	20.3	11.5	37.8	8.9	26.5
General Practitioners	14.0	12.0	3.9	14.3	5.6	8.5
Specialists:						
Actual	50.7	37.0	39.3	62.5	21.7	52.5
Expected on Basis of Specialty	50.9	49.6	51.1	52.9	53.6	52.5
Actual Minus Expected	-0.2	-12.6	-11.8	9.6	-31.9

of Loyola. Loyola, a Roman Catholic University, produces a much larger percentage of physicians of Irish, Italian, and Polish extraction than it does of Jewish and Anglo-Saxon physicians; therefore this variable should be considered in analyzing specialization in each ethnic group.

When the percentage of specialists is compared with the percentage expected on the basis of medical school attendance, the high percentage of Anglo-Saxon physicians who specialize can be explained simply in terms of their selection of schools (Table 5). Jewish physicians, although showing a rather high expected percentage of specialization, have an even higher proportion actually specializing. By contrast, Irish, Italian, and Polish physicians do not specialize to the degree expected on the basis of their medical school rates. Furthermore, members of the two groups exceeding their own ethnic population's needs are more likely to specialize than the total medical profession in Chicago, whereas the remaining three groups have rates lower than the city-wide rate.

Loop Practice. This aspect of medical practice requires separate analysis of general practitioners and specialists since the latter, regardless of ethnic membership, locate in the Loop more frequently than general practitioners. The analysis of specialists is complicated by the fact that certain specialties are more likely to be practiced in the Loop than others; thus, in the random sample, only four of the eighteen offices of pediatricians were in the Loop, compared with twenty of the twenty-five offices of

psychiatrists and/or neurologists. Although the numbers are too small for detailed analysis, there is some evidence that physicians of different ethnic groups vary in the specialties that they practice. Consequently, the expected percentages of specialists in the Loop were computed using the rates of Loop practice for each specialty in the random sample.

For both specialists and general practitioners, Jewish physicians were the most likely to practice in the Loop, followed in descending order by Anglo-Saxons, Irish, and Italians and Poles. As in the case of specialization, the actual rate for Anglo-Saxon specialists was no different from their expected rate. Irish, Italian, and Polish specialists were considerably below their expected percentage in the Loop.

It appears, then, that physicians who are members of groups that are overrepresented in medicine are more likely to specialize and to practice in the Loop. Since such practices are assumed to involve higher prestige than general practice in a local neighborhood, physicians who must compete for out-group patients probably are more likely to need and use these devices for status reinforcement. No doubt some of the differences in these rates are due to the differential demand of the ethnic groups for particular services from their doctors, but further research would be required to examine this alternative interpretation. The approach used in this study seems to be supported by data reported by Hughes, that physicians of English stock practicing in the "French-Canadian" city of Montreal were twice as spe-

cialized as their counterparts in the "English" cities of Toronto and Vancouver, but that in all three cities roughly the same percentage of physicians were specialized.¹⁵

CONCLUSION

This paper has considered some effects of the auxiliary status of ethnic membership

¹⁵ Everett C. Hughes, *French Canada in Transition*, Chicago: University of Chicago Press, 1943, p. 208. Cf. Oswald Hall, "The Informal Organization of Medical Practice in an American City," Ph.D. dissertation, University of Chicago, 1944, pp. 211-212.

upon the practice of medicine. Notwithstanding its methodological shortcomings and the fact that the interpretation involves certain assumptions which deserve separate examination, the data strongly suggest that the ethnic identification of physicians significantly influences their pattern of medical practice. Both the spatial distribution and the functional differentiation of physicians reported here suggest that medicine may be viewed as a system resulting from and concordant with the more specialized and segregated services performed by each component of the medical profession.

FERTILITY THROUGH TWENTY YEARS OF MARRIAGE: A STUDY IN PREDICTIVE POSSIBILITIES

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How well can the number of children born in the first 20 years of marriage be predicted from a wide range of sociological and psychological characteristics of the couple measured before marriage? This question represents the primary focus of this paper. In particular, there are three aspects of the problem to which the analysis is directed:

(1) Studies involving the prediction of family size frequently have relied upon statements of desired family size as an index of actual fertility. A recent report¹ concluded that asking couples before marriage the number of children they wanted resulted in the explanation of only seven per cent of the variation of actual fertility. The authors at that time claimed that certain sociological and psychological variables were better predictors of family size than was this direct approach. What are these variables and how

much of an improvement in prediction results?

(2) It is assumed that desired family size reflects sets of predisposing social and psychological antecedents. Does it follow that these variables measured prior to marriage predict fertility in the same manner as desired family size, or are other dimensions relating to fertility independently involved? One approach to this question compares factors affecting total fertility with those relevant to the size of planned families, after the influence of variations in contraceptive practice is removed.

(3) Finally, what is the structure of relationships among the variables affecting fertility, variables which range over such nominally discrete categories of economic, social-familial, and personality characteristics?

THE DATA

Data that would yield unequivocal answers to these questions are lacking. The results presented in this paper are based upon information collected by Kelly in a longitudinal

¹ Charles F. Westoff, Elliot G. Mishler, and E. Lowell Kelly, "Preferences in Size of Family and Eventual Fertility Twenty Years After," *American Journal of Sociology*, 62 (March, 1957), pp. 491-497.

study of marital adjustment covering a total period of 20 years.² Since the main interest of the Kelly research is marital adjustment, the variables included did not reflect hypotheses derived from theories of differential fertility. However, the number and kinds of measured variables permit an examination of many factors cited in previous research as well as variables heretofore not included in fertility studies, particularly those in the area of personality, at least some of which appear to be theoretically relevant.

The original sample consisted of 300 engaged couples. The present analysis is based on 145 couples, who represent the residue following the elimination of couples if no marriage had resulted, if one or both spouses had been married more than once, had reported problems of sterility, had adopted foster children, or had returned insufficient information.

In view of these eliminations and the selective nature of the original sample, which revealed a very high incidence of college attendance and a low proportion of Catholics,³ the results of this analysis are limited in generality. The primary advantage of these data derive from the unique longitudinal design of the study, the first opportunity to assess the level of prediction possible over a long period of time, and the hypotheses suggested by the analysis.⁴

THE VARIABLES

The dependent variable with which we are concerned is the total number of live births to the couple. It is reasonable to assume after 20 years of marriage and the couples' reported preferences for additional children, that well over 95 per cent of all births

² For further details, see E. Lowell Kelly, "Consistency of the Adult Personality," *American Psychologist*, 10 (November, 1955), pp. 659-681.

³ Many of the couples were originally located through newspaper announcements of engagements and invited to cooperate. Approximately 65 per cent of the women had at least some college education; Roman Catholics constituted 10 per cent of the total sample.

⁴ Some of these hypotheses have been incorporated in a new study of American fertility. See Elliot G. Mishler and Charles F. Westoff, "A Proposal for Research on Social Psychological Factors Affecting Fertility: Concepts and Hypotheses," in *Current Research in Human Fertility*, New York: Milbank Memorial Fund, 1955, pp. 121-150.

to these couples has already occurred. Therefore we can safely conclude that reported births closely approximate completed family size.

A number of key variables are of particular interest. Education and socio-economic background have long been major factors in the study of differential fertility.⁵ The traditional inverse relationship between these variables and fertility has been changing during the past two decades, with the more educated and white-collar classes manifesting sharp increases in fertility.⁶ Since most of the respondents in this study were married between 1934 and 1939, they fall within the last pre-war marriage cohort. Moreover, as noted above, the sample is heavily overrepresented with the college educated. Although the group is not a truly representative sample, its composition has particular interest to observers of the modern fertility trend.⁷

Another area of interest (provoked by the Indianapolis Study) is the analysis of fertility at the individual psychological level, particularly in the area of personality.⁸ This orientation is reflected in the content of the new study and its preliminary field test.⁹ A number of themes are represented, one relating to the level of generalized manifest anxiety. The hypothesis is that

⁵ See, for example, Clyde V. Kiser and P. K. Whelpton, "Fertility Planning and Fertility Rates by Socio-Economic Status," in P. K. Whelpton and Clyde V. Kiser, editors, *Social and Psychological Factors Affecting Fertility*, Vol. 2, New York: Milbank Memorial Fund, 1950, pp. 359-415.

⁶ See Charles F. Westoff, "Differential Fertility in the United States: 1900 to 1952," *American Sociological Review*, 19 (October, 1954), pp. 549-561; "College Study Report—1956," *Population Bulletin*, 12 (October, 1956), pp. 89-108.

⁷ For a provocative, speculative discussion of the causes of the new fertility pattern among the college educated, see Elbridge Sibley, "Higher Education and Earlier Parenthood: A Changing Cycle of Family Life," *The Antioch Review*, (Spring, 1957), pp. 45-59.

⁸ The Indianapolis Study included hypotheses on the relation of fertility and fertility planning to such variables as feelings of personal inadequacy, ego-centered interest in children, and fear of pregnancy and childbirth.

⁹ Frank W. Notestein, Elliot G. Mishler, Robert G. Potter, Jr., and Charles F. Westoff, "Pretest Results of a New Study of Fertility in the United States," *Proceedings of the International Statistical Institute, Stockholm*, 1958 (in press).

women who are overly concerned with their own personal problems will lack ego resources sufficient to afford the psychological costs posed by the demands of children for attention and care and, thus, will be likely to have fewer children. The type of data collected by Kelly provides a further opportunity to test this hypothesis. Both the Bernreuter Personality Inventory and the Bell Adjustment Inventory were included among the battery of tests administered to these couples before marriage and both contain measures of feelings of personal inadequacy and unsuccessful adjustment to a variety of social situations.

Another variable that has intrigued students of fertility is the effect on family size of the number of siblings the parent had as a child. The hypothesis that family size is directly related over generations has been tested in two recent large-scale fertility studies, being definitely confirmed in a British study¹⁰ but questioned in the Indianapolis investigation.¹¹ The data analyzed below permit an examination both of the predictive potentialities of this variable as well as the relationships among couples who have successfully planned their families.

THE ANALYSIS

With these variables in mind but with a more general exploratory attitude toward the data as well, we undertook a series of correlation analyses with total fertility as the dependent variable. Some selected results are presented in Table 1. It is apparent that the educational and socio-economic variables correlate positively with fertility although the values are low. The size of family in which the wife was raised correlates positively (.29) with the number of children she herself has had after 20 years of marriage. As the subsequent analysis indicates, the number of siblings she had as a child appears to affect her

TABLE 1. CORRELATIONS* OF SELECTED FAMILY BACKGROUND AND PERSONAL CHARACTERISTICS OF THE WIFE MEASURED BEFORE MARRIAGE WITH TOTAL FERTILITY AFTER TWENTY YEARS

Variable	r
Family of Origin:	
Socio-economic status** of parents	.20
Prestige rating *** of father's occupation	.13
Average income over last 3 years	.12
Education of father	.23
Education of mother	.17
Size of parental family	.29
Personal Characteristics:	
Education	.18
Otis Test of Mental Ability	.24
Age at marriage	-.25
Number of children desired	.27
Neurotic tendency (Bernreuter)	-.22
Introversion	-.14
Dominance	.18
Inferiority	-.19
Emotional adjustment (Bell)	.20
Social adjustment	.19

* Pearsonian product-moment correlations are based on an N of 145.

** The index of socio-economic status was coded by the method described in J. Crosby Chapman and V. M. Sims, "The Quantitative Measurement of Certain Aspects of Socio-Economic Status," *Journal of Educational Psychology*, 16 (1925), pp. 380-390. The index measures characteristics of the home such as having a telephone, automobile, central heating, piano; and level of intellectual interests as reflected in numbers of newspapers and magazines taken and books owned.

*** Prestige scores assigned from N.O.R.C. scales.

own fertility by directly influencing the number of children she wants to have by the time she is married.

Six subtests of the Bernreuter and Bell inventories show the expected correlations with fertility. The general pattern revealed is for neurotic tendency, introversion, submissiveness, inferiority, and poor emotional and social adjustment to predict low fertility. The question immediately provoked by this pattern is whether these tests are measuring different variables or are different labels for the same personality characteristic. The intercorrelations¹² reveal that probably no

¹⁰ Jerzy Berent, "Relationship between Family Sizes of Two Successive Generations," *Milbank Memorial Fund Quarterly*, 31 (January, 1953), pp. 39-50.

¹¹ John F. Kantner and Robert G. Potter, Jr., "The Relationship of Family Size in Two Successive Generations," in P. K. Whelton and Clyde V. Kiser, editors, *Social and Psychological Factors Affecting Fertility*, Vol. 4, New York: Milbank Memorial Fund, 1954, pp. 1069-1086.

¹² The intercorrelations among the four Bernreuter tests and the "emotional adjustment" scale of the Bell Adjustment Inventory average .80; the two scales of the Bell inventory correlate at .37, and the intercorrelation of the social adjustment scales with the Bernreuter scales averages .69. For

TABLE 2. INTERCORRELATIONS AMONG NINE SELECTED INDEPENDENT VARIABLES AND STANDARDIZED BETA WEIGHTS OF MULTIPLE CORRELATION ANALYSIS WITH TOTAL FERTILITY

Variables	2	3	4	5	6	7	8	9	Beta Weights
1. Number of children desired	-.15	.24	-.13	.06	.12	.07	.24	.29	.19
2. Neurotic tendency (Bernreuter)		-.20	-.64	-.03	-.08	-.20	-.22	-.09	-.05
3. Otis Test of Mental Ability			.15	-.02	.41	.28	.56	-.03	.13
4. Social adjustment (Bell)				-.07	.15	.11	.30	.04	.13
5. Age at marriage					-.06	-.07	.12	-.02	-.23
6. Socio-economic status of parents						.54	.42	-.06	.04
7. Education of father							.37	-.02	.13
8. Education of wife								.02	-.03
9. Size of parental family									.23

more than two factors are being tapped by these six scales, with the measures of "neurotic tendency" from the Bernreuter¹³ and "social adjustment" from the Bell inventory appearing to be the best representatives of each.

In summary, the correlations presented in Table 1 suggest that high fertility is linked with a good socio-economic family background, better education and mental ability test performance, coming from and wanting a large family, emotional stability and successful social adjustment, and an early age at marriage.

Two related questions may now be raised. How much variation in fertility can be attributed to these variables collectively, and to what extent do they overlap or reflect a smaller number of common factors? In order to answer these questions, a matrix of intercorrelations was computed which represents a reduced matrix from the original list of variables (see Table 2). The reduction was achieved by reference to two criteria: variables were eliminated if their intercorrelations were so high as to suggest they were probably different measures of the same phenomenon, as in the case of some of the personality measures noted above; and only

more extensive evidence of the homogeneity of these Bernreuter scales, see Glenn C. Martin, "A Factor Analysis of the Bernreuter Personality Inventory," *Educational and Psychological Measurements*, 8 (Spring, 1948), pp. 85-92; and Donald E. Super, "The Bernreuter Personality Inventory: A Review of Research," *Psychological Bulletin*, 39 (February, 1942), pp. 94-125.

¹³ See the summary of evidence on reliability and validity in Robert G. Bernreuter, *Manual for the Personality Inventory*, Stanford: Stanford University Press, 1935.

variables with comparatively high correlations with fertility were retained.¹⁴

Multiple correlation analysis, involving the variables in Table 2, was employed to determine the amounts of variation in fertility accounted for by particular combinations of independent variables. Three multiple correlations were computed. The first, involving all nine variables in Table 2, resulted in a correlation of .52 or about 27 per cent of the variation in fertility.

Examination of the standardized beta weights indicates that neurotic tendency, socio-economic status of parents, and education of wife add little to the predictive battery. The multiple correlation based on the remaining six independent variables remains substantially the same. To assess the power of these six variables to predict fertility, in comparison with the respondents' own statements prior to marriage of the number of children they wanted, a third multiple correlation was computed excluding this latter variable. This value is .49. Thus, about 24 per cent of the variation of fertility is accounted for by the five variables of size of parental family, education of father, mental ability test score, the social adjustment scale, and age at marriage. This is an appreciable improvement over the 7 per cent controlled by family-size preferences stated before marriage. Even combining the husband's and the wife's family-size preference increases this prediction only from 7 to 9 per cent.

¹⁴ Admittedly, this practice of culling high correlations, especially from a large pool of possible candidates, is unsound statistical practice. In this case, it was used only as a basis for suggestive avenues of further investigation.

The fact that three theoretically relevant variables add little in the way of predictive efficiency, although each of them correlates with fertility, raises questions about the structure of the relationships among the variables. In spite of the criterion of retaining variables with low intercorrelations, there must still be redundancies among the predictors. A factor analysis,¹⁵ with the five extracted factors rotated orthogonally, revealed that the main redundancies lie in the socio-economic status of parents and the father's education; the respondent's education and her score on the Otis Mental Ability test; the number of children desired and size of parental family; and neurotic tendency and social adjustment. The fifth factor, accounting for half of the common factor variance of fertility, was defined solely by age at marriage.

It would seem profitable to identify further the factors isolated in this *ad hoc* analysis. To this point the analysis emphasizes the relevance of five factors in explaining fertility. Clearly, there are hosts of additional variables involved since three-quarters of the variance in total fertility is not accounted for by these predictors. Three possible explanations may be offered. First, the fact that the variable of completed fertility is quantitative and orders nicely (0, 1, 2, 3, etc.) does not imply that the psychological intervals between children are equal or that the same factors are relevant at all stages. A serious question can be raised about the expectation of high correlations with such a variable when we know that the specific factors affecting behavior with respect to the third child, for example, may not be relevant in the case of either the next or the previous child. The very process of building a family has self-generating consequences for attitudes toward having more children; for example, the sex distribution of the children or sibling relationships may outweigh more general factors in the determination of ultimate family size.

Secondly, the dependent variable of fertility reflects the values of both wife and hus-

band. A prediction from the characteristics of one to the exclusion of the other undoubtedly reduces the maximum prediction possible.¹⁶

Finally, the number of children a couple has by the end of 20 years of marriage obviously depends on a wide variety of factors. Aside from physiological factors (some of which have been eliminated by the purification of the sample), there is the whole range of cultural and motivational variables which affect not only desires about the number and spacing of births but the regularity and effectiveness of contraceptive practice. To the extent that different sets of factors determine the number and spacing of planned births, on the one hand, and the efficacy of contraceptive behavior, on the other, the prediction of total fertility will be depressed.¹⁷

The data permit partial investigation of the husbands' characteristics as well as factors relating to contraceptive behavior. Only an analysis dealing with the size of planned families, however, is reported in this paper.

SIZE OF PLANNED FAMILIES

The analysis thus far has been confined to the completed fertility of all 145 couples whatever the efficiency of family planning. In order to eliminate the difference in fertility due to varying degrees of unsuccessful planning, the sample was reduced to 68 couples who reported practicing contraception with complete success.¹⁸ The analysis then proceeded in a manner similar to that used for the total sample, with the objectives

¹⁵ For a discussion and evidence of the merits of treating the couple as the basic statistical unit in studies of fertility, see Robert McGinnis, "Similarity in Background Characteristics and Differential Fertility," *Social Forces*, 34 (October, 1955), pp. 67-72.

¹⁶ A crude measure of fertility-planning success was devised by dichotomizing the sample into couples reporting that all pregnancies were completely planned and those who admitted that at least one of their pregnancies was conceived while contraception was actually being practiced or at a time when they "forgot" or took a chance. This classification which split the sample approximately in half correlated at - .42 with total fertility (the successful planners having smaller families).

¹⁸ That is, they either practiced contraception continuously and had no pregnancies (2 couples) or interrupted contraception deliberately in order to conceive.

¹⁵ The complete centroid method of Thurstone was used. Since the results of the factor analysis corroborated the multiple correlation analyses, only elucidating the patterns of redundancy, the detailed results have not been reproduced here.

of isolating the variables which appear to be predictors of successfully planned fertility and examining the structure of interrelationships. The main difference in approach, since information collected at the last contact with the couples was used to classify fertility-planning success, is that interest resides in the internal analysis of factors affecting planned fertility rather than in the predictive power of these variables over a twenty year period. Since it is reasonable to assume, however, that the future will be characterized by increasing voluntary control of child-spacing and family size, multiple correlation analysis comparable to that employed for the total sample was repeated.

A comparison of the variables relating to planned fertility with those relating to total fertility reveals four common to both cases: desired family size, size of parental family, age at marriage, and neurotic tendency. All of these, except the first, have relationships of approximately the same value. As might be expected, the correlation between desired family size and actual fertility is higher in this group of successful planners than for the total sample, revealing a value of .45 compared to .27 (among unsuccessful planners it drops to .19).

Five other variables correlate with fertility in this restricted group which do not differentiate fertility within the whole group. Two of these, a happy childhood and a home in which discipline is remembered as infrequent, associate with high fertility. Women who were active in extracurricular activities in school and college tend to have more children. The final pair of variables, sociability and optimism,¹⁹ also reveal a direct relationship with fertility.

Educational level and socio-economic background are conspicuously absent in this part of the analysis. The correlations observed between these variables and size of planned family are negligible. Moreover, the relationship between these variables and fertility-planning success, though small, is negative; that is, couples from a better socio-economic environment and with higher education

tended to have more unplanned births. A similar inverse association was observed in the pretest of a new study of social and psychological factors affecting fertility. Although the evidence on this question is conflicting, one hypothesis to be tested with data from the new study is that the direct relationship between various measures of socio-economic status and fertility does not demand a theory which postulates an increase in the desire for children, but can be at least partly explained by a reduction of contraceptive vigilance resulting from a relaxation of financial pressures. If this is correct, one would expect that the better-educated classes and the more economically secure would be inclined to take more "chances." There are a number of important qualifications that must be attached to this hypothesis, for example, the relevance of different stages in the family-building process (chance-taking may operate mainly in the earlier stages) and the recognition that such a relationship is not inconsistent with an over-all picture of a positive relation between education and the proportion of deliberately planned pregnancies.

The level of prediction of planned fertility is higher than that calculated for the total sample. The multiple correlation involving all nine independent variables is .66, a control of 44 per cent of the variance. This higher value is due partly to the greater predictive power of family-size desires for this homogeneous group; eliminating this variable, the correlation drops to .59.²⁰

The intercorrelations in Table 3 again suggest the possibility of the presence of a smaller number of factors which might account for the variations in planned fertility. To clarify the structure of relationships, a complete centroid factor analysis was performed with a total of four factors being extracted. The objective of the orthogonal rotations was to achieve simple structure while allowing the variance of fertility and desired family size to distribute among the factors as they would. The identification of each of the four factors can be inferred from the rotated structure (first half of Table 4).

Factor I is by far the most important

¹⁹ Scales were derived by averaging the rating of several acquaintances of the respondent. See E. Lowell Kelly, "A 36 Trait Personality Rating Scale," *Journal of Psychology*, 9 (January, 1940), pp. 97-102.

²⁰ Corrections for shrinkage, giving unbiased estimates of R, would result in somewhat smaller values of these estimates.

TABLE 3. INTERCORRELATIONS * AMONG TEN SELECTED VARIABLES FOR THE SUB-GROUP OF SUCCESSFUL PLANNERS

Variables	2	3	4	5	6	7	8	9	10
1. Fertility	.45	.34	.22	-.23	-.26	.23	.22	-.25	.23
2. Desired family size		.23	.07	-.11	-.15	.04	-.01	-.16	.08
3. Size of parental family			-.14	-.03	-.15	.22	.03	-.11	-.03
4. Childhood happiness				-.34	.06	.05	.20	-.23	.07
5. Frequency of home discipline					.11	-.10	-.05	.02	.14
6. Age at marriage						-.01	.07	-.02	.01
7. Sociability							.62	-.10	-.01
8. Optimism								-.16	-.02
9. Neurotic tendency (Bernreuter)									-.47
10. Extra-curricular school activities									

* Pearsonian product-moment correlations, based on an N of 68.

for planned fertility. Its structure reveals significant loadings on three other variables and indicates a pattern of high fertility, desire for a large family, growing up in a large family, and an early age at marriage. These variables, it will be recalled, are three of the best predictors of fertility for the total group.

Factor II, with a loading of .34 on planned fertility, is defined by memories of a happy childhood and infrequent punishment. Factor III, with no loading on fertility, is defined exclusively by a factor common to ratings as a sociable and an optimistic person. Finally Factor IV, containing a loading of .42 on fertility, can be identified as a factor common to scoring low in the Bernreuter Neurotic Tendency scale and having been active in school and college extracurricular activities; this is possibly an extroversion factor.

No serious attempt has been made to attach labels to these factors since this analysis is designed to delineate merely the bare out-

lines of the underlying structure of variables relating to fertility. Without further analysis, which would involve many other criterion-variables and additional systematic factor studies, the conceptual distinction between these last two factors (III and IV) cannot be further clarified. A more conclusive identification of their content and the nature of their motivational connections with fertility decisions requires further research.

SUMMARY AND CONCLUSIONS

A number of different though related questions have been posed in this analysis. The main interest lies in the unique longitudinal design of the study which permitted an examination of the extent to which fertility can be predicted over virtually the entire span of the marital reproductive period. From data collected prior to marriage, excluding any knowledge of contraceptive practice, 27 per cent of the variation in fertility was accounted for by nine variables.

TABLE 4. THE ORTHOGONALLY ROTATED * AND CENTROID FACTOR STRUCTURES **

Variables	Rotated Loadings				h^2	Centroid Loadings			
	I	II	III	IV		I	II	III	IV
Fertility of planners	.74	.34	.04	.42	.84	.81	.17	-.28	-.28
Desired family size	.46	.14	-.11	.20	.28	.38	.12	-.29	-.20
Size of parental family	.50	-.17	.11	.14	.30	.32	.37	-.25	.04
Childhood happiness	-.11	.67	.02	.12	.48	.36	-.42	.32	-.26
Frequency of home discipline	-.17	-.54	-.03	.09	.33	-.30	.10	-.24	.41
Age at marriage	-.37	-.03	.11	-.01	.15	-.17	-.17	.20	.22
Sociability	.16	.07	.80	.17	.71	.51	.35	.44	.36
Optimism	.00	.18	.71	.14	.55	.43	.18	.49	.31
Neurotic tendency	.11	-.15	-.03	-.80	.68	-.53	.45	.22	-.38
Extra-curricular school activities	-.09	-.05	-.13	.60	.38	.26	-.33	-.34	.29

* Graphic solution.

** Communalities estimated by method of successive iterations.

Eliminating four variables, including the respondents' statements of family-size preferences, reduced the level to only 24 per cent. This final battery is composed of size of parental family, father's education, mental ability test score, social adjustment, and a young age at marriage—all of which correlate directly with fertility. If it can be assumed that chance relationships have not been capitalized on in the selection of these independent variables, the results confirm the hypothesis that certain sociological and psychological factors are better predictors of fertility than the wife's own family-size preference expressed before marriage.²¹ To some extent, the data support the contention that desired family size reflects sets of predisposing social and psychological antecedents consistent with achieved fertility. The more accurate prediction of the size of planned families suggests the relevance of information on fertility planning. One of the possible ways of improving the prediction of fertility would seem to be to concentrate on the correlates of successful fertility planning. The factor analyses suggest the

existence of at least three distinct factors accounting for the variance of planned fertility. Predictive ability is also likely to increase from the identification and elaboration on these and additional factors. The fact that there are unconsidered factors is apparent since most of the variance is not explained. Moreover, the selective nature of the sample and other conditions imposed on the analysis which limit prediction are sufficient to restrict the extent to which this result can be generalized. The analysis is intended only to confirm the possibilities of such prediction and to suggest avenues for more systematic investigation.

In this latter connection, the analysis particularly of those of these factors affecting the size of planned families has led to the inclusion in a new study of hypotheses relating fertility and family-size preferences to such variables as the number of siblings, hedonic tone of childhood, and generalized anxiety feelings. Many of the hypotheses focus on certain social-psychological factors presumed to be involved in the complex connections among socio-economic status and education, the formulation of family size desires, the use and effectiveness of contraception, and fertility.

²¹ Even combining the husband's family-size preference with his wife's increased this prediction from 7 to only 9 per cent.

POSTMARITAL CONSEQUENCES OF PREMARITAL SEX ADJUSTMENTS

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THE aims of this study are to explore sexual adjustment before and during the honeymoon period¹ and to examine postmarital consequences of premarital coitus with spouse during this period. The honeymoon represents a phase of marriage that has been the subject of much humor, of considerable morbid speculation, but of

little research.² Much of the existing knowledge concerning this early marital period is in the form of case materials which too often stress the more dramatic and anomalous factors influencing honeymoon sex adjustment. Some of the factors publicized as playing a central role in early marital sex adjustment are examined here. Our hypothe-

¹ The honeymoon period is limited here to the first two weeks of marriage. This span of time was selected because it frequently represents the period of the wedding trip, if one is taken.

² Only one paper could be found devoted to research on the subject: Stanley R. Brav, "Note On Honeymoons," *Marriage and Family Living*, 9 (Summer, 1947), pp. 60-65.

sis is that sexual adjustment during this early period is strongly influenced by the degree of premarital intimacy experienced with spouse; and more specifically, that not only sexual satisfaction but the folkways of the wedding trip and the postmarital employment of contraception, are affected significantly by premarital sex behavior with spouse.

The universe of subjects for this study consisted of 190 married women, wives of students residing in three housing units on a midwestern university campus. Each subject was personally contacted and the nature of the research was briefly explained to her; she was then requested to complete a four-page schedule. Anonymity of response was guaranteed by special techniques.³ The attempt to obtain complete coverage of this universe was fairly successful: only nine of the subjects either refused to cooperate or could not be located, and four schedules had to be discarded on the basis of incompleteness. The remaining 177 completed schedules were analyzed. These couples represent a relatively homogeneous group with reference to certain characteristics: they were young and fairly recently married (mean years of marriage 4.3); they were above average in education (mean years of college education 2.5 for the wives and 3.1 for the husbands); and 96 per cent were Protestants.

PREMARITAL INTIMACY

On one point, our data show a striking agreement with the Burgess and Wallin investigation which found premarital coitus with spouse reported by approximately 45 per cent of the couples studied.⁴ Premarital

³ The respondents were presented with an envelope and a schedule and instructed that upon completion the schedule was to be sealed in the envelope and placed in a briefcase containing other sealed envelopes.

⁴ Ernest W. Burgess and Paul Wallin, *Engagement and Marriage*, New York: J. B. Lippincott Co., 1953, pp. 330-331. Initially it was decided that data concerning premarital coitus with persons other than spouse would be necessary for full understanding of the problem. However, in many instances the wife completed the schedule in the presence of her husband, and although he was unable to view her responses, his presence seems to have resulted in a high frequency of "blanks" for items probing other premarital contacts.

coital experience with spouse was indicated by 43.5 per cent of the 177 respondents in the present study.

Information on the age and education of the respondents failed to show a significant relationship with premarital coitus with spouse. However, the findings concerning the relationship between church attendance and coital experience were significant. Pairs in which neither spouse reported regular church attendance at the time of marriage were most apt to report intercourse. Premarital experience was indicated by 28.2 per cent of the 85 pairs of regular churchgoers, by 47.7 per cent of the 44 pairs composed of one regular and one non-regular attender, and by 61 per cent of the 48 homogamous non-attending pairs ($\chi^2=18.72$; d.f.=2; $P<.001$).

There are few data in the literature on the relationship between social class and premarital coitus with spouse. But previous investigations have demonstrated the cogent influence of social class on other forms of sexual behavior;⁵ the latter suggests a possible class differential with respect to premarital intimacy with spouse. Such differentials were found in the present study. The occupational status of the father, determined by Warner's occupational scale,⁶ was employed as an index of social class of the respondents and their husbands. Of the 171 women whose social class could be determined, 41.5 per cent fell into the upper-middle class, 45.0 per cent into the middle class, and 13.5 per cent into the lower class. The majority (82.6 per cent) of lower class women were involved in premarital coital behavior; females from the upper-middle class indicated the smallest incidence (31.0 per cent); with middle class females in an intermediate position with 41.6 per cent

⁵ See Alfred C. Kinsey, et al., *Sexual Behavior in the Human Male*, Philadelphia: W. B. Saunders Co., 1948, and *Sexual Behavior in the Human Female*, Philadelphia: W. B. Saunders Co., 1953.

⁶ W. Lloyd Warner et al., *Social Class in America*, Chicago: Science Research Associates, Inc., 1949, pp. 140-141. Because of the size of our N the seven categories of the scale were telescoped into three grouping: 1 and 2; 3, 4, and 5; and 6 and 7—to be referred to as upper-middle (categories 1 and 2 were composed mainly of professionals and did not represent a true upper class), middle, and lower class, respectively.

TABLE 1. COMPARATIVE SOCIAL CLASS AND PREMARITAL COITUS WITH SPOUSE *

	Male Higher Class	Male Same Class	Male Lower Class
Premarital Coitus:			
Yes	56.0%	40.6%	32.7%
No	44.0	59.4	67.3
Total Percentage	100.0	100.0	100.0
Total Number	(50)	(69)	(52)

$\chi^2=6.09$; d.f.=2; .05>P>.02.

* N=171 since 6 respondents failed to include sufficient information to determine social class.

claiming a coital history ($\chi^2=18.28$; d.f.=2; $P<.001$). No association was found between the husband's social class and the degree of premarital intimacy with spouse. The class positions of husbands, however, do become important when compared with the class positions of their wives.⁷ The analysis of comparative social class (see Table 1) reveals premarital intercourse to be most characteristic of couples where the male is of a higher class. A smaller incidence of sexually intimate behavior is observed for pairs of equal class composition, and there is even less where the male is of a lower social class—what are the implications of these data?

It has been the practice to speak of *exploitation* when males were found to have entered sexual liaisons with women of comparative lower status; and it might be argued that our data suggest such exploitation. But exploitation can be considered from two points of view. On the one hand, male exploitation for sexual gratification may lead to emotional or legal involvement and subsequent marriage. Female exploitation, on the other, may consist of granting sexual privileges as a means of insuring further involvement, subsequent marriage, and vertical mobility. This latter type may be particularly evidenced by lower class females, whose more pronounced premarital sexual accessibility may be due to the nature of a

⁷ See Winston Ehrmann, "Influence of Comparative Social Class of Companion Upon Premarital Heterosexual Behavior," *Marriage and Family Living*, 17 (February, 1955), pp. 48-53; Eugene J. Kanin, "Male Aggression in Dating-Courtship Relations," *American Journal of Sociology*, 63 (September, 1957), p. 199.

socialization process that does not provide her with alternative qualities and techniques of attraction. Girls from the upper-middle and middle classes are not as prone to dispense sexual favors to the point of coitus. Possibly, they can afford to be more provocative and designing with their sexuality by exploiting other available assets such as "personality," "culture," and "charm."⁸ At the same time, of course, class sex codes play an important role in determining sexual accessibility. In American society, the female maintains primary control over the sexual destiny of the pair relationship, withholding or granting sexual privileges. Our data suggest that the value she places on her virginity—a factor which strongly influences her course of action—is class-bound to a great extent.

HONEYMOON PERIOD

The effects of premarital intercourse may be manifested in the question of whether or not a couple believe it necessary to formalize the honeymoon period with a wedding trip. The function of the wedding trip is ordinarily thought of as providing the newly married pair with the opportunity to make the transition into the married state with the greatest facility. In our society, the sexual component of this transition is frequently, if not always, considered highly dramatic and of great importance for the newlyweds. On this basis, it could be expected that couples who feel sexually adjusted (or at least sexually initiated prior to marriage) would be least interested in formalizing this adjustment period with a wedding trip. This hypothesis is borne out. The findings show that about 47 per cent of the 77 couples who had premarital intercourse also had a wedding trip in contrast with 87 per cent of the 100 pairs not experiencing premarital intercourse ($\chi^2=35.23$; d.f.=1; $P<.001$).⁹

⁸ For a more developed discussion of this point of view see Albert W. Cohen, *Delinquent Boys*, Glencoe, Ill.: Free Press, 1955, pp. 144-147.

⁹ A. B. Hollingshead reports a similar behavior pattern with respect to marital status. Where both pair members were entering matrimony for the first time 94.5 per cent reported a wedding trip whereas such a trip was indicated by only 61.5 per cent of the pairs composed of previously married members. "Marital Status and Wedding Be-

TABLE 2. PREMARITAL COITUS AND CONTRACEPTIVE USAGE DURING THE FIRST TWO WEEKS OF MARRIAGE

	No Premarital Coitus	Premarital Coitus
Contraception:		
Not employed	26.0%	46.8%
Employed	74.0	53.2
Total Percentage	100.0	100.0
Total Number	(100)	(77)

$\chi^2=8.18$; d.f.=1; .01>P>.001.

This pattern tended to persist regardless of social class, suggesting that the wedding trip is most apt to be taken by those pairs for whom it can serve the traditional function.

The folkway of contraception might also be thought of as receiving differential response from couples with and without prior coital experience. Some method of contraception was found to have been employed by almost 64 per cent of the couples during this early marital period. It should not be assumed that the remainder sought immediate conception since 47 per cent of the non-contraceptors indicated a definite antipathy for pregnancy during this time. Disregard for contraceptive measures can be related to two important variables, social class and premarital intercourse.

As we would expect, contraceptive usage varies according to social class. About two-thirds of the couples with a middle or upper-middle class spouse (husband or wife) employed contraception, while less than one-third of the couples with a lower class spouse were contraceptors (28.6 per cent of those with lower class husbands and 30.4 per cent of those with lower class wives). Thus, the practice of contraception among these couples is associated with the social class background of either the husband or the wife.

A significant relationship was also found between premarital intercourse and lack of contraceptive usage during the honeymoon period (see Table 2). Coital experience prior to marriage may dispose some couples to feel "ready" for children, with erotic desires gratified to some degree. And non-contraceptive premarital relations not resulting in pregnancy may lead to feelings of

behavior," *Marriage and Family Living*, 14 (November, 1952), p. 311.

security against unwanted pregnancies. Of course, premarital pregnancy also may contribute to disregard for contraception. In addition, the more daring couples who participated in premarital intercourse might be expected to manifest less caution as well in such matters as planning parenthood.

Further examination of the data indicates that both social class and premarital sexual experience probably influenced contraceptive behavior. Of the couples with upper-middle class wives who had premarital coital experience, 55.5 per cent were found to have employed contraception in early marriage as compared to about 78 per cent of those without premarital coitus. In the middle class group, 62.5 per cent of the pairs with premarital coitus were contraceptors in contrast to over 71 per cent of the pairs without such coitus. For pairs with lower class wives, about 26 per cent of the initiated and one-half of the uninitiated were contraceptors.

In the family literature, immediate postmarital sexual problems have been attributed to many factors. Writers debunking the honeymoon on the ground that it follows strenuous wedding preparations, long receptions, and exhausting trips, have suggested that the wedding night is often doomed to failure as a direct consequence of these fatiguing activities. Our findings, however, fail to show any relationship between length of reception or of travel and ratings of satisfaction with the first coital experience in marriage. This holds for couples with and without premarital intercourse. Of course, this statistical relationship does not preclude the possibility that individual cases are seriously affected by such factors.

Furthermore, it is frequently claimed that the "artificiality" and the general stresses and strains of the formalized honeymoon period contribute to sexual maladjustment. Classification of couples on the basis of whether or not they went on a honeymoon trip failed to reveal any relationship between this factor and the wife's satisfaction with the first two weeks of sexual experience. The present findings suggest that if the formalized honeymoon is an agent of maladjustment, it is doubtful that the area of sex relations is affected.

Our data indicate an association between premarital intercourse with spouse and full

sex expression during the early weeks of marriage.¹⁰ The much popularized wedding night difficulties obviously should be analyzed on the basis of whether or not the married pair has experienced coitus prior to marriage. Comparatively, sexual satisfaction was readily attained on the wedding night, by wives who had experienced premarital intercourse. Using a four-point scale, about 71 per cent of the females who had premarital intercourse with spouse reported wedding-night relations as "very satisfying" or "satisfying,"¹¹ in contrast with 47 per cent of the females without premarital experience.

Sexual difficulties have often been emphasized as being of prime importance during the honeymoon period.¹² Among the couples in our study, 47.5 per cent of the wives experienced difficulties of some kind during the first two weeks of marriage. Of these 84 wives, some 58 per cent, reported difficulties of a sexual nature and about 42 per cent were concerned with non-sexual matters. For these women, a comparison drawn between the type of difficulty and the level of premarital erotic intimacy shows that sexual difficulties were mentioned by three fourths of the wives with premarital coital experience but only by 43 per cent of the uninitiated ($X^2=10.07$; d.f.=1; $.01>P>.001$).

It may appear paradoxical that the wives who experienced coitus prior to marriage also indicated the greatest amount of sexual difficulties immediately after marriage. A partial explanation may be that these initiated women may entertain higher expectations of immediate postmarital sex adjustment. Perhaps, then, if more complete adjustment

is expected, sexual problems of lesser importance are reported as "difficulties." Women without premarital coital experience, on the other hand, probably do not have such high expectations of immediate postmarital sex adjustment and hence their sexual problems may be considered more "natural." Such an interpretation receives support from our data. When the respondents who reported sexual difficulties were divided on the basis of whether their first two weeks of sexual activity were "satisfying" or "not satisfying," it was found that although women with premarital experience claimed more difficulties, about 87 per cent of them also indicated that their sexual experiences were satisfying, as compared with only 31.5 per cent of the women without premarital coitus ($X^2=13.54$; d.f.=1; $P<.001$). Undoubtedly, the sexual difficulties reported by the women with premarital experience were of a different nature.

Immediate postmarital sexual difficulties also could be largely dependent upon whether or not orgasm was achieved during premarital intercourse. Of the 43 women who reached orgasm from premarital coitus, only 28 per cent reported sexual difficulties, whereas such difficulties were admitted by 47 per cent of the women who failed to achieve orgasm from premarital intercourse. Although these data are in the expected direction, they are not statistically significant ($X^2=3.65$; d.f.=1; $.10>P>.05$). The occurrence of orgasm from noncoital sex play does not appear related to early marital sex adjustment. Of the 74 women who "petted" but failed to experience orgasm, 19 per cent reported sex difficulties, but difficulties were also indicated by 15 per cent of the 26 women who achieved orgasm from petting.

Orgasm from premarital intercourse apparently also influences the wife's rating of satisfaction of the wedding night to a much greater degree than does orgasm brought about by petting. The initial sex experience in marriage was reported as satisfying by over 81 per cent of the wives experiencing premarital coital orgasm as compared to only 46 per cent of those indicating petting-determined orgasms ($X^2=7.13$; d.f.=2; $.05>P>.02$).

Not all degrees of premarital intimacy

¹⁰ Burgess and Wallin, *op. cit.*, pp. 362-363. See Kinsey, . . . *Human Female*, *op. cit.*, p. 406; Robert L. Hamblin and Robert O. Blood, "Premarital Experience and the Wife's Sexual Adjustment," *Social Problems*, 4 (October, 1956), pp. 122-130; Harriet R. Mowrer, "Sex and Marital Adjustment: A Critique of Kinsey's Approach," *Social Problems*, 1 (April, 1954), pp. 147-152.

¹¹ Orgasm is probably the best single criterion of sexual gratification. However, because such an early view of sex in marriage was asked of respondents, it was decided to use "satisfying," which may not imply orgasm at all.

¹² Cf. Brav., *op. cit.*, p. 60. Sexual difficulties were indicated by nearly all of the 56 per cent of the wives in Brav's sample who listed some difficulty.

TABLE 3. WIVES' EVALUATIONS OF MAXIMUM LEVELS
OF PREMARITAL INTIMACY FOR IMMEDIATE
POSTMARITAL SEX ADJUSTMENT

	Kissing and Petting Above the Waist	Genital Petting	Coitus
Helping sex adjustment	33.9%	54.5%	58.4%
Not helping sex adjustment	66.1	45.5	41.6
Total Percentage	100.0	100.0	100.0
Total Number	(56)	(44)	(77)

$\chi^2=8.69$; d.f.=2; .02>P>.01.

are considered equally conducive to early marital sex adjustment.¹³ Table 3 shows that the more advanced the intimacy level, the more likely the subjects considered it "beneficial" for the attainment of early marital sex adjustment. Women whose maximum premarital intimacy levels were kissing or petting above the waist were least inclined to ascribe favorable consequences to these activities for marital sex adjustment. It is interesting to note that in spite of the fact that coitus was considered most beneficial, approximately 42 per cent of the women with such premarital experience declined to attribute a beneficial effect to the experience. Although coitus was not subjectively viewed as paying greater dividends than genital petting, the ratings of sexual satisfaction for the wedding night imply otherwise. Reluctance to ascribe positive effects to premarital intercourse may reflect guilt and regret over such behavior. On the other hand, as noted above, those women with premarital sexual episodes tend to report sexual difficulties in the early weeks of marriage. Again, expectations of sexual adjustment could be high and subsequent difficulties, even of a minor nature, might be interpreted as evidence that premarital coitus is largely void of benefit for marital sexual adjustment.

Frequently the honeymoon period is portrayed as a time when the married pair may encounter difficulties which have long-lasting or permanent effects. In our sample, only 14 women (7.9 per cent) were con-

scious of long term difficulties which began during the first two weeks of marriage. It is somewhat perplexing that these 14 respondents also had histories of premarital intercourse. Hence, 18.2 per cent of those with premarital coitus reported current marital difficulties which were initiated during this early phase of marriage. Of these 14 women, five reported that they felt like "sex servants," four indicated that wedding night confessions of prior sexual indiscretions were still plaguing their marriages, three mentioned a general sexual incompatibility, and two reported nonsexual problems. This breakdown means that only 4.5 per cent of the total group actually reported difficulties stemming from sexual incompatibility. There is little support here for the claim that sexual problems encountered during the early part of marriage frequently carry over into later married life. Of course, the ramifications of these early difficulties may not be clearly perceived by these respondents.

Other evidence is also somewhat contradictory to the purportedly powerful influence of the initial coital episode in marriage. A bungling of the wedding night may provide the hapless groom with a frigid wife, so some "sexologists" maintain. Our data show that while the first coital episode in marriage was reported as either "unsatisfying" or "very unsatisfying" by 42.4 per cent of the women studied, only 17 per cent evaluated the total sexual experience of the first two weeks of marriage in this way. Again, it is necessary to separate the respondents on the basis of premarital intimacy. Table 4 indicates that although the decrease of unfavorable ratings is high for both groups, this decrease is most pronounced for women who had experienced premarital coitus. At the end of the first two weeks of marriage, only 7.8 per cent of the women with premarital coitus report their sexual activities as unsatisfying, in contrast with 24 per cent of the women without such experience. Women with premarital experience appear to achieve sexual satisfaction in marriage more readily. This capacity for earlier sexual responsiveness may be due to more premarital coital experience and, perhaps more important, personalities which are to some degree free of inhibiting normative

¹³ *Ibid.* Brav reports that only 10 per cent of his respondents considered premarital experience helpful during the honeymoon.

controls, as evidenced by the lack of religious interests.

TABLE 4. WIVES' RATINGS OF SATISFACTION FOR INITIAL COITUS IN MARRIAGE AND FOR COITAL EXPERIENCE OF THE FIRST TWO WEEKS OF MARRIAGE, ACCORDING TO DEGREE OF PREMARITAL INTIMACY

	Initial Coitus in Marriage		Coital Experience of Immediate Post- Marital Period	
	No Pre- marital Coitus	Premarital Coitus	No Pre- marital Coitus	Premarital Coitus
Very satisfying	18.0%	32.5%	34.0%	55.8%
Satisfying	29.0	38.9	42.0	36.4
Not satisfying	49.0	23.4	24.0	7.8
Very unsatisfying	4.0	5.2
Total Percentage	100.0	100.0	100.0	100.0
Total Number	(100)	(77)	(100)	(77)

SUMMARY

The present findings on the honeymoon period indicate the importance of premarital intimacy with spouse not only in terms of immediate postmarital sexual satisfaction but also in terms of altered postmarital behavior. The ritualized change of status, signified by the wedding trip, was attributed greatest importance by the uninitiated. Similarly, the practice of contraception also received a more favorable response from couples without premarital experience. Although sexual satisfaction obtained on the wedding night and during the honeymoon period was associated with premarital intimacy, a considerable amount of sexual adjustment was reported to have occurred within a short span of time for the virginal wives, despite the numerous difficulties indicated for the wedding night.

It should be emphasized that the foregoing findings do not imply the rapid attainment of complete sex adjustment in marriage, merely an individual satisfaction.

MARITAL ADJUSTMENT: A FACTOR ANALYSIS STUDY*

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MARITAL adjustment has been defined as the presence of such characteristics in a marriage as a tendency to avoid or resolve conflicts, a feeling of satisfaction with the marriage and with each other, the sharing of common interests and activities, and the fulfilling of the marital expectations of the husband and wife. Operationally it is generally defined as that which is measured by a marital-adjustment test. The latter consists of a number of questions which ask husbands and wives how they perceive or assess their marriages.

* The authors express their appreciation to Paul R. Christensen and James W. Frick for their statistical computations and valuable suggestions; to James A. Peterson for his help in assembling the original data; and to J. P. Guilford, Donald Van Arsdol, and particularly Edgar F. Borgatta for criticizing the manuscript.

Adjustment in marriage, of course, is different from prediction of marital adjustment. A marital-prediction test indicates the expected degree of adjustment in marriage at a given time, whereas marital adjustment is a cross-sectional picture of adjustment at a given time. Marital adjustment is neither a description of the predictive factors associated with adjustment in marriage nor a measure of the causative factors that produce marital adjustment.

The earliest, and probably the only, prior attempt to apply factor analysis to marital items was published by Burgess and Cottrell in 1939.¹ Using 28 of their 97 marital-prediction variables, they found five clusters or

¹ Ernest W. Burgess and Leonard S. Cottrell, *Predicting Success or Failure in Marriage*, New York: Prentice-Hall, 1939, pp. 317-322.

factors that were predictive of adjustment in marriage for their sample. In 1945 Burgess and Locke indicated that factor analysis would be of assistance in determining if there is a general factor as well as several specific factors in the marital-adjustment test, but they made no study to test this hypothesis.² In 1952 Faris stated that happiness in marriage might be reducible to six or seven factors.³ This paper reports an exploratory attempt to identify through factor analysis the clusters of items or factors in a marital-adjustment test.

We consider three questions concerning the factors in the empirically developed marital-adjustment test: (1) Are there items in the test which are doing no work, that is, have no significant loadings on any factor of marital adjustment? (2) Do the items cluster together and measure one general factor or do they cluster around several more specific factors? (3) Are the factors independent, with the items measuring only one factor, or do the items spread over various factors?

THE TEST SUBJECTED TO FACTOR ANALYSIS

The test used in the factor analysis is based on earlier studies of several investigators. The earliest, published in 1924, was that of Hamilton who used 13 questions to measure satisfaction in marriage.⁴ The marital-adjustment test of Burgess and Cottrell was composed of 19 major items, plus seven incidental questions; four of their major items were borrowed from Hamilton's test.⁵ Terman used 15 of the 19 major items of the Burgess-Cottrell study, adding three additional items.⁶ Locke employed all 19 major items of the Burgess-Cottrell test plus ten of his own.⁷ A joint study by Peterson and

² Ernest W. Burgess and Harvey J. Locke, *The Family: From Institution to Companionship*, First Edition, New York: American Book Company, 1945, p. 448.

³ Robert E. L. Faris, *Social Psychology*, New York: Ronald Press, 1952, p. 404.

⁴ G. V. Hamilton, *A Research in Marriage*, New York: Albert and Boni, 1929, pp. 60-76 and 80.

⁵ Ernest W. Burgess and Leonard S. Cottrell, *op. cit.*, pp. 64-65.

⁶ Lewis M. Terman, *Psychological Factors in Marital Happiness*, New York: McGraw-Hill, 1938, Chapter 4, particularly p. 58.

⁷ Harvey J. Locke, *Predicting Adjustment in*

Williamson,⁸ selecting 20 questions from Locke's test as a measure of marital adjustment,⁹ provides the test subjected to the factor analysis in the present investigation.

Table 1 gives the 20 items, the "favorable" and "unfavorable" responses to each, and the per cent of the sample which gave the favorable responses.

THE SAMPLE

The cases subjected to factor analysis consist of 171 husbands and 178 wives,¹⁰ of the 420 spouses interviewed by Peterson and Williamson; for greater homogeneity persons 50 years of age and older were eliminated. The original sample came from three social areas of Los Angeles: a lower-class area, with 63 couples; a lower-middle-class area, with 82 couples; and an "upper-middle-class" area, with 65 couples.¹¹

The couples to be interviewed were selected on a random basis within each of the three social areas. Husbands and wives were interviewed separately in their homes. A male interviewed the husband and a female interviewed the wife.

The average age of the sample was 33 years. Over 20 per cent were reared in rural areas, and 30 per cent in cities of under 100,000 population. Approximately one-fifth of the couples had been married more than once. The median education was about 12

Marriage: A Comparison of a Divorced and a Happily Married Group, New York: Henry Holt and Company, 1951, Chapter 3, particularly pp. 45-52.

⁸ James A. Peterson, "The Relation of Objective and Subjective Factors to Adjustment and Maladjustment in Marriage," and Robert C. Williamson, "Socio-Economic Factors in Marital Adjustment in an Urban Setting," Ph.D. dissertations, University of Southern California, 1951.

⁹ The length of the questionnaire made it advisable to use this abbreviated test. Nine items were eliminated from Locke's test because they duplicated other items in the test, did not discriminate effectively between adjusted and unadjusted marriages, or might have interfered with rapport in the interview.

¹⁰ Most of these were husbands and their wives. Husbands and wives were included in a single sample, since investigators have found only small correlations between the responses of husbands and wives.

¹¹ Robert C. Williamson, "Socio-Economic Factors in Marital Adjustment in an Urban Setting," *American Sociological Review*, 19 (April, 1954), pp. 213-216.

TABLE 1. MARITAL-ADJUSTMENT ITEMS, FAVORABLE AND UNFAVORABLE RESPONSES, AND PER CENT RESPONDING FAVORABLY

Item	Favorable Response	Unfavorable Response	Per Cent of Sample Responding Favorably
1. Check the place on the scale which best describes the degree of happiness of your present marriage.	checking 4 or 5	checking 1, 2, or 3	58
1 2 3 4 5			
Very Happy Very Happy			
Unhappy			
2. Check any of the following things which you think cause serious difficulties in your marriage: attempt to control my spending money, insincerity, excessive criticism, narrowmindedness, untruthfulness, paying attention to another person, being easily influenced by others, religious differences, different amusement interests, lack of mutual friends, ill health, constant bickering, lack of mutual affection, selfishness, adultery, etc.	0	1 or more	60
3. How often do you and your mate "get on each other's nerves?"	never, rarely	occasionally, frequently	49
4. Do you ever wish you had not married?	never	rarely, occasionally, frequently	36
5. If you had your life to live over, do you think you would:	marry the same person	marry a different person, not marry at all	88
6. When disagreements arise, they usually result in:	agreement by mutual give and take	husband giving in, wife giving in, neither giving in	73
State approximate extent of agreement or disagreement during marriage on the following items:			
7. Handling family finances	always agree	do not always agree *	40
8. Matters of recreation	always agree	do not always agree	32
9. Religious matters	always agree	do not always agree	65
10. Amount of time spent together	always agree	do not always agree	54
11. Choice of friends	always agree	do not always agree	44
12. Sex relations	always agree	do not always agree	45
13. Ways of dealing with in-laws	always agree	do not always agree	43

* Includes almost always agree; occasionally, frequently, almost always, and always disagree.

TABLE 1. MARITAL-ADJUSTMENT ITEMS, FAVORABLE AND UNFAVORABLE RESPONSES, AND PER CENT RESPONDING FAVORABLY—(Continued)

Item	Favorable Response	Unfavorable Response	Per Cent of Sample Responding Favorably
14. Conventional behavior	always agree	do not always agree	45
15. Aims, goals, and things believed important in life	always agree	do not always agree	36
16. Do you confide in your mate?	always	almost always, rarely, almost never	36
17. In leisure time, husband and wife both prefer:	to stay at home	to be on the go, or one prefers to be on the go and the other to stay at home	42
18. Do you and your mate engage in outside activities together?	all of them	some, very few, none of them	35
19. Do you kiss your mate?	every day	occasionally, almost never	86
20. What things does your mate do that you do not like?	nothing, one thing, two things	three or more things	64

years. A third were manual workers, a third were lower-white-collar employees, and a third were in upper-white-collar and professional occupations. The couples were predominantly Protestant.

METHODS

The items were intercorrelated on the basis of the dichotomy described in Table 1. Tetrachoric correlation coefficients were computed with the cosine pi formula, with corrections being applied where the point of dichotomization was sufficiently different from .50. The matrix of intercorrelations is shown in Table 2.

Eight factors were extracted from the matrix of intercorrelations by Thurstone's complete centroid method. After the eighth centroid factor was extracted, the residual correlations had a leptokurtic distribution about a mean of zero, and the highest absolute value was less than .20.

An orthogonal rotation was made by means of the analytic method suggested by Peters

and Van Voorhis.¹² This technique was used because we were looking for independent and stable dimensions or factors. The results are included in Table 3.

It was decided to include only items with a loading of .30 or higher. Five of the eight factors have several items with such loadings and are discussed below.

FINDINGS

An analysis of the items in Table 3 reveals some substantial loadings in the rotated matrix. Although these clusters of items cannot be assumed to be causes of marital adjustment, they reveal some of the factors contained in the marital-adjustment test.

It will be noted that some items are loaded in two or more of the five factors. It also will be noted that two names are given to the factors. Any labeling of a factor is arbitrary,

¹² C. C. Peters and W. R. Van Voorhis, *Statistical Procedures and Their Mathematical Bases*, New York: McGraw-Hill, 1940.

TABLE 2. CORRELATION MATRIX OF ITEMS FROM THE MARITAL-ADJUSTMENT TEST*

Favorable (Positive) Response	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
a. Sex: Male **	.06	.10	.08	.10	-.04	.18	-.21	-.19	.05	-.21	.21	-.14	-.10	.36	.22	-.23	.08	-.15	.03	.00
1. Marriage very happy	.50	.53	.57	.61	.19	.19	.14	.14	.22	.15	.33	.22	.21	.33	.30	.12	-.01	.28	.27	
2. No marital difficulties	.53	.50	.63	.39	.20	.18	.13	.28	.28	.32	.28	.23	.29	.23	-.01	.19	.39	.43		
3. Never-rarely get on each other's nerves	.61	.44	-.33	.16	.26	.18	.29	.20	.29	.34	.28	.34	.32	.14	.23	.36	.26			
4. Never wish had not married	.74	.37	.34	.44	.23	.47	.38	.44	.32	.39	.50	.58	-.05	.21	.27	.33				
5. Marry the same person	.41	.10	.38	.23	.37	.29	.46	.34	.30	.41	.38	.08	.37	.51	.33					
6. Agree on mutual give and take	-.20	.09	.15	.15	.13	.00	.14	.18	.17	.17	.17	-.09	-.12	.45	.35					
7. Agree on finances	.44	.36	.36	.43	.42	.45	.52	.48	.34	-.04	.11	.09	.03							
8. Agree on recreation	.41	.54	.58	.54	.28	.51	.52	.35	.04	.31	.18	.09								
9. Agree on religion	.26	.33	.35	.36	.41	.56	.10	.17	.08	.05	.11									
10. Agree on time spent together	.56	.40	.29	.58	.55	.33	.04	.44	.09											
11. Agree on friends	.54	.38	.53	.50	.30	-.02	.28	.01	.15											
12. Agree on sex relations	.47	.50	.50	.25	.13	.30	.25	.17	.08											
13. Agree on in-laws	.39	.39	.17	.15	.10	.11	.11													
14. Agree on conventional conduct	.64	.34	.09	.33	.37	.21														
15. Agree on aims and goals	.32	.10	.21	.20	.25	.17														
16. Always confide in mate	.04	.43	.16	.13																
17. Both prefer to stay at home	.22	-.12	.05																	
18. Engage in all outside activities together	.22	.07																		
19. Kiss every day																				
20. Mate does nothing you dislike																				

* Decimal points deleted.

** Male plus, female minus.

TABLE 3. ROTATED FACTOR MATRIX *

Favorable (Positive) Response	Companionship		Agreement	Affectional Intimacy	Masculine Interpretation			
	A	B			E	F	G	H
a. Sex: Male **	-28	.05	.09	.40	.21	-.01	.22	.13
1. Marriage very happy	.27	.09	.40	.05	.58	-.01	.09	-.12
2. No marital difficulties	.21	.24	.46	-.12	.25	.05	-.06	-.20
3. Never or rarely get on each other's nerves	.00	.23	.60	.04	.37	-.52	-.12	-.19
4. Never wish had not married	.14	.49	.16	.00	.65	.12	-.33	-.10
5. Marry the same person	.45	.31	.36	.08	.46	.11	.10	-.18
6. Agree by mutual give and take	.44	.00	-.10	-.19	.29	.67	.18	.15
7. Agree on finances	-.21	.60	.27	-.02	.09	.00	-.02	.23
8. Agree on recreation	.02	.58	.21	.28	.17	.19	-.04	.20
9. Agree on religion	.19	.64	-.02	-.17	.04	.09	.14	.08
10. Agree on time together	-.10	.52	.28	.40	.11	.16	-.03	.03
11. Agree on friends	-.25	.66	.16	.27	-.07	.25	.04	-.13
12. Agree on sex relations	.16	.51	.47	.20	.09	.21	-.05	-.15
13. Agree on in-laws	.13	.55	.29	-.09	.01	.03	.01	-.02
14. Agree on conventional conduct	.38	.54	-.03	.42	.23	.07	.03	-.20
15. Agree on aims and goals	-.02	.69	.23	.6	.28	.15	.26	.01
16. Always confide in mate	.00	.22	.17	.32	.65	.02	-.04	.13
17. Both prefer to stay at home	.34	.18	-.11	.17	-.07	-.29	.14	-.13
18. Engage in all outside activities together	.31	.14	.30	.61	.08	-.07	-.02	.15
19. Kiss every day	.38	-.12	.45	-.18	.45	.20	.16	.05
20. Mate does nothing you dislike	.14	.14	.17	-.07	.20	.03	.21	-.11

* Decimal points deleted.

** Male plus, female minus.

of course, the important matter being the clustering together of items.

justifying, perhaps, "couple-sufficiency" as an appropriate label.

Factor A

This factor is labeled *Companionship* or "*Couple-Sufficiency*." It is composed of the following six items:

- 5. Marry the same person if life to live over.. .45
- 6. Agree by mutual give and take..... .44
- 14. Agree on conventional conduct..... .38
- 19. Kiss every day..... .38
- 17. Both prefer to stay at home..... .34
- 18. Engage in all outside activities together.... .31

Companionship is implied in wishing to marry the same person if one had his life to live over, agreement by mutual give and take in settling disputes, and engaging in all outside interests together. The preference to stay at home rather than be "on the go" is one item that appears only in this factor,

Factor B

This factor is called *Agreement* or *Consensus*. Eleven items cluster together in this factor. Six of these (finances, recreation, religion, choice of friends, in-laws, and aims and goals believed important in life) appear only in this factor.

- 15. Agree on aims, goals..... .69
- 11. Agree on friends..... .66
- 9. Agree on religion..... .64
- 7. Agree on finances..... .60
- 8. Agree on recreation..... .58
- 13. Agree on in-laws..... .55
- 14. Agree on conventional conduct..... .54
- 10. Agree on time spent together..... .52
- 12. Agree on sex relations..... .51
- 4. Never wish one had not married..... .49
- 5. Marry same person if life to live over.... .31

There is little doubt that this factor implies a strong similarity in tastes, outlook, and especially in values. That a husband and wife always agree may be due to a near-identity of traits before they married, or may be brought about by the process of growing together after marriage. It is interesting that of these questions the most heavily loaded is agreement on "aims, goals, and things believed important in life"; the least loaded is agreement about sex relations.

Factor C

This factor is labeled *Affectional Intimacy* or *Emotional Adjustment*. It is composed of seven items:

- | | |
|---|----|
| 3. Never or rarely get on each other's nerves... | 60 |
| 12. Agree on sex relations..... | 47 |
| 2. No marital difficulties..... | 46 |
| 19. Kiss every day..... | 45 |
| 1. Marriage very happy..... | 40 |
| 5. Marry same person if life to live over..... | 36 |
| 18. Engage in all outside activities together.... | 30 |

The fact that the responses to these seven items cluster together raised the problem of appropriate designation. Affectional intimacy is implied by kissing every day and agreement on sex relations, while "Never or rarely get on each other's nerves" has the highest loading and implies emotional adjustment.

Factor D

This factor is termed *Masculine Interpretation* or *Wife Accommodation*. It is composed of four items from the marital-adjustment test and one designating the sex of the respondent:

- | | |
|---|----|
| 18. Engage in all outside activities together.. | 61 |
| 14. Agree on conventional conduct..... | 42 |
| a. Sex, male..... | 40 |
| 10. Agree on time spent together..... | 40 |
| 16. Always confide in mate..... | 32 |

Male or female was included as a supplementary item and it appears only in Factor D. This means that this factor is apparently the only one that elicited different responses from wives and husbands. The husbands were more likely to respond favorably on these items; wives presumably were accommodative. Burgess and Cottrell also report that, even among their white-collar,

upper-educated sample, wives had to make the major adjustments in marriage.¹³

Factor E

This factor is called *Euphoria* or *Halo Effect*. It has six items:

- | | |
|---|----|
| 4. Never wish had not married..... | 65 |
| 16. Always confide in mate..... | 65 |
| 1. Marriage very happy..... | 58 |
| 5. Marry same person if life to live over.... | 46 |
| 19. Kiss every day..... | 45 |
| 3. Never or rarely get on each other's nerves.. | 37 |

Factor E seems to be composed of items toward which people have an euphoric tendency. In other words, some persons react to these questions with great enthusiasm and perceive all marital relationships as perfect.

Three additional factors were extracted from the matrix of intercorrelations. One of these, Factor F, has two loadings, both relatively high:

- | | |
|--|----|
| 6. Agreement by mutual give and take..... | 67 |
| 3. Never or rarely get on each other's nerves. — | 52 |

It will be noted that the second correlation is negative, which may mean that persons admit they get on each other's nerves and yet settle agreements by mutual give and take.

The other two factors, G and H, have one and no loadings, respectively, at the selected .30 level.

CONCLUSIONS

The questions raised at the beginning of this paper may be answered as follows:

1. Only one item in the test has no loadings at the selected level on any factor of marital adjustment, namely: What things does your mate do that you do not like?

2. The items cluster around several factors. Factor A has 6 items; B, 11; C, 7; D, 5; and E, 6. Thus on four of the factors there is a fairly even number of loaded items. It will be remembered that 6 of the 11 items in Factor B appear only in this factor and all refer to marital agreements. This may mean that the number of items on agreement could be reduced without detracting from the value of the test.

¹³ Ernest W. Burgess and Leonard S. Cottrell, *op. cit.*, pp. 341-343

3. While the factors appear to be sufficiently well defined to be identified in the orthogonal rotations, independent tests for the factors cannot be defined for this sample of items and this sample of subjects.

One item has loadings on four of the factors studied: if you had life to live over you would marry the same person. Three items appear on three factors: agreement on conventional conduct, engage in outside activities together, and kiss every day. Six items have loadings on two factors, and nine have loadings on only one factor. The spread of some of the items over more than one factor suggests that they may be general indexes of marital adjustment.

This analysis of the 20-item marital-adjustment test indicates that the test that has developed from empirical investigations is composed of several factors. It appears, how-

ever, that it would be possible to refine the test, first, by deleting item 20 which had no loadings at the .30 level on any of the factors of marital adjustment; second, by expanding Factor F; and, third, by including certain other factors, particularly one relating to sex-relations.

Finally, marital adjustment should be redefined in terms of the interrelated variables derived from the factor analysis. Marital adjustment is an adaptation between husband and wife to the point where there is companionship, agreement on basic values, affectional intimacy, accommodation, euphoria, and certain other unidentified factors. Studies should be made to discover the unidentified factors in marital adjustment and to validate these by comparing sub-scores with independent criteria.

RESEARCH REPORTS AND NOTES

COMPONENTS OF POPULATION CHANGE
IN LARGE METROPOLITAN SUBURBS

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In a major research project recently completed, Bogue used the vital statistics method to estimate the "components" of population change (natural increase and net migration) in metropolitan central cities and their suburban rings in the most recent intercensal decade, 1940-50.¹ His results indicate that the growth of central cities was dependent largely upon natural increase rather than migration. Many individual central cities, in fact, appear to have lost substantial numbers through migration. Presumably, most of these migrants moved to the adjacent ring areas, participating in the decentralization process. These metropolitan rings were growing rapidly as a result of both natural increase and net in-migration, with migration making the major contribution.

The research reported here represents an attempt (1) to extend this general mode of analysis to the larger incorporated suburban places lying within the rings of the largest metropolitan cities, and (2) to determine the components of population change in two functional types of suburb—"employing" and "residential" suburbs. Employing suburbs are functionally similar to metropolitan central cities in that they are "attracting" areas, drawing large numbers of workers to them every day. In contrast, residential suburbs are "dispersing" areas, sending their residents to work in other parts of the metropolitan area.²

¹ Donald J. Bogue, *Components of Population Change, 1940-1950: Estimates of Net Migration and Natural Increase for Each Standard Metropolitan Area and State Economic Area*, Oxford, Ohio and Chicago: Scripps Foundation for Research in Population Problems and Population Research and Training Center, University of Chicago, 1957. Preliminary results may be found in Donald J. Bogue and Emerson Seim, "Components of Population Change in Suburban and Central City Populations of Standard Metropolitan Areas, 1940 to 1950," *Rural Sociology*, 21 (September-December, 1956), pp. 267-275.

² This distinction is elaborated in Leo F. Schnore, "The Functions of Metropolitan Suburbs," *American*

Prior research indicates rather clearly that the aggregate growth of residential suburbs during the last two decades was well beyond that of employing suburbs.³ Moreover, this overall differential in favor of residential suburbs tended to persist when other relevant variables were controlled, and was interpreted as primarily the result of different patterns of migration. Specifically, it was suggested that many employing suburbs—like many central cities—were absorbing migration losses in the decade 1940-1950, with only their natural increase preventing overall population declines. On the other hand, it was suggested that residential suburbs—like metropolitan rings in general—more frequently gained population from both sources (natural increase and net migration), with migration predominating.⁴ In other words, it was hypothesized that areas exhibiting broad functional similarities tend to experience analogous patterns of population change.

Thus the research reported here is essentially an addendum to the writer's previous study of suburban growth differentials according to functional type, and it represents a further exploration of one question that arose in the course of that work. Although the results of the present study are inconclusive, it seems ap-

can Journal of Sociology, 61 (March, 1956), pp. 453-458. The operational definition of employing and residential suburbs is based upon the "employment-residence ratio" computed by Victor Jones for all incorporated places of 10,000 or more inhabitants that lie within the "rings" of Standard Metropolitan Areas. It is simply the ratio of (1) the number of people employed in the suburb to (2) the number of employed residents of the suburb, and is computed by the formula $(1) + (2) \times 100$. Suburbs with a ratio of 85 or above are identified as "employing" subcenters, with all suburbs having a lower ratio classified as "residential." see Victor Jones, "Economic Classification of Cities and Metropolitan Areas," in *The Municipal Year Book*, 1953, Chicago: International City Managers' Association, 1953, pp. 49-57.

³ Leo F. Schnore, "The Growth of Metropolitan Suburbs," *American Sociological Review*, 22 (April, 1957), pp. 165-173; see also Chauncy D. Harris, "Suburbs," *American Journal of Sociology*, 49 (July, 1943), pp. 1-13.

⁴ Leo F. Schnore, "Satellites and Suburbs," *Social Forces*, 36 (December, 1957), pp. 121-127.

proper to present them (in summary form) on the assumption that even negative findings may prove to be of utility to others conducting research in this field. Some value may also derive from another demonstration of the possibly misleading effect of weighted averages in the study of population growth.

METHODS, SCOPE, AND LIMITATIONS

The basic technique was the vital statistics method of estimating net migration. For each separate suburban place in the study, natural increase or decrease in the intercensal decade (April 1, 1940–April 1, 1950) was estimated by reference to vital statistics reports of births and deaths as allocated to places of residence. The difference between this estimate and overall population change, 1940–1950, was then taken as an estimate of net migration.

It must be emphasized that this procedure yields only crude estimates of natural increase and net migration. In addition to errors arising from under-registration of births and deaths, "particularly for intra-metropolitan studies, one must be aware of the substantial errors in reporting place of residence."⁵ These "errors of allocation" are difficult and often impossible to determine in retrospect. Close inspection of the data, however, indicated no reason to believe that these errors were more prevalent in one or the other type of suburb; thus hidden errors of under-registration and allocation should not seriously bias the comparison of the two types. As in Bogue's study, adjustments were made for changes in the official census treatment of college students.⁶ But unlike Bogue's study, no account was taken of changes either in military population or those resulting from annexation. Changes in military population were relatively infrequent in the suburbs investigated, and annexation posed no great problem since only a handful of these large and relatively stable areas experienced boundary changes affecting population.⁷

⁵ Otis Dudley Duncan, "Research on Metropolitan Population: Evaluation of Data," *Journal of the American Statistical Association*, 51 (December, 1956), p. 592.

⁶ Corrections were made by reference to estimates contained in the *Rand McNally Commercial Atlas and Marketing Guide*, 1952, Chicago: Rand McNally and Co., 1952, pp. 40–41.

⁷ That areal changes played a relatively minor role is indicated by the fact that only 11 of 68 employing suburbs and 8 of 67 residential suburbs identified in this study made any annexations or detachments in the decade under consideration. The removal of these 19 cases in no way altered the inferences drawn for any of the five metropolitan areas, for the direction of differences between types

TABLE 1. COMPONENTS OF POPULATION CHANGE IN EMPLOYING AND RESIDENTIAL SUBURBS OF THE FIVE LARGEST STANDARD METROPOLITAN AREAS OF THE UNITED STATES, 1940–1950

Component of Population Change	Employing Suburbs	Residential Suburbs	All Suburbs
A. Per cent change in population, 1940–1950			
Total change	14.2	25.7	18.8
Natural increase	11.5	12.0	11.7
Net migration	2.7	13.7	7.1
B. Total change according to components (per cent)			
Total change	100.0	100.0	100.0
Natural increase	80.7	46.8	62.1
Net migration	19.3	53.2	37.9
Number of places *	68	67	135

* All incorporated places of 10,000 or more inhabitants (1940) in the following Standard Metropolitan Areas: New York—Northeastern New Jersey, Chicago, Los Angeles, Philadelphia, and Detroit.

But the use of the vital statistics method imposes another important limitation, for the study is necessarily confined to those incorporated places that had a population of at least 10,000 in 1940. Vital statistics in the inter-censal period were not reported separately for unincorporated places or for incorporated places below this size limit. In one sense, however, this limitation can be viewed as something of an advantage, since it introduces at least a crude control over suburban size—one of the variables previously shown to be associated with overall growth within both suburban types.⁸ Finally, because of the relatively small number of metropolitan suburbs meeting the criteria for inclusion, the study is confined to an examination of residential and employing suburbs in only the five largest metropolitan areas—New York, Chicago, Los Angeles, Philadelphia, and Detroit. These were the only areas having central cities of at least one million inhabitants in both 1940 and 1950.

FINDINGS

Table 1 presents the basic findings in summary form, with the large suburbs of all five areas

of suburb was not affected. Area data were drawn from the following sources: for 1940 *The Municipal Year Book, 1941*, Chicago: International City Managers' Association, 1941, Table II, pp. 43–80; for 1950, U. S. Bureau of the Census, "Land Area and Population of Incorporated Places of 2,500 or More: April 1, 1950," *Geographic Reports*, No. 5 (January, 1953).

⁸ "The Growth of Metropolitan Suburbs," *op. cit.*

combined. The upper panel shows percentage changes attributable to the two components in the two types of suburb. It is evident that estimated growth by natural increase (11.5 per cent) was greater than that by net migration (2.7 per cent) in the employing suburbs, while the opposite holds in residential suburbs, where net migration (13.7 per cent) slightly exceeded natural increase (12.0 per cent). The lower panel of Table 1 indicates the proportion of the total growth in the two suburban types that can be attributed to these two sources of growth. About four-fifths of the gain in employing suburbs came from natural increase, in contrast with less than half in residential suburbs.

These results are clearly in accord with the substantive hypotheses outlined above, for the two suburban types are clearly differentiated with respect to the major sources of their growth. However, Table 2—an area-by-area extension of the upper panel of Table 1—indicates that these generalizations cannot be extended to all of the metropolitan areas in the study.

TABLE 2. COMPONENTS OF POPULATION CHANGE IN EMPLOYING AND RESIDENTIAL SUBURBS, IN THE NEW YORK, CHICAGO, LOS ANGELES, PHILADELPHIA, AND DETROIT STANDARD METROPOLITAN AREAS, 1940-1950

Area and Component of Population Change	Per Cent Change, 1940-1950, by Type		
	Employing Suburbs	Residential Suburbs	All Suburbs
New York	7.2	12.3	9.3
Natural increase	9.3	8.8	9.1
Net migration	-2.1	3.5	0.2
Chicago	12.2	13.3	12.6
Natural increase	13.1	10.1	11.9
Net migration	-0.9	3.2	0.7
Los Angeles	39.7	54.9	47.8
Natural increase	11.6	16.2	14.0
Net migration	28.1	38.7	33.8
Philadelphia	6.7	16.7	8.0
Natural increase	10.7	10.9	10.7
Net migration	-4.0	5.8	-2.7
Detroit	28.4	49.3	37.3
Natural increase	22.2	23.6	22.8
Net migration	6.2	25.7	14.5
Number of places	68	67	135
New York	31	29	60
Chicago	12	14	26
Los Angeles	11	12	23
Philadelphia	10	4	14
Detroit	4	8	12

Examination of the first column on *employing* suburbs reveals a clear exception in the Los Angeles area, where growth by net migration was clearly in excess of natural increase. Moreover, the second column of Table 2, showing components of growth in *residential* suburbs, reveals three exceptions to the general pattern indicated by Table 1. Growth through natural increase was clearly in excess of growth through net migration in the residential suburbs of New York, Chicago, and Philadelphia—the older areas of slower expansion. The expected pattern can be found only in Los Angeles and Detroit—the two large areas growing most rapidly in the 1940-1950 decade. These findings suggest that the growth of the metropolitan area as a whole may be a crucial factor in determining the patterns found in its constituent parts.

More importantly, this area-by-area comparison demonstrates that the aggregate results shown in Table 1 hide a great deal of variation. The rates shown there are nothing more than *weighted averages*—averages that tend to conceal more than they reveal. Moreover, even the data for individual metropolitan areas shown in Table 2 are somewhat misleading, at least to the extent that they imply uniformity of growth according to type *within* areas. Table 3 suggests the extent to which *individual suburbs* vary with respect to growth by natural increase and net migration. This summary table shows the frequency distribution of percentage changes, according to component and type of suburb, for individual places, indicating considerable variation within each type, variation that is concealed in Tables 1 and 2.

CONCLUSIONS

This analysis of components of growth in the two types of metropolitan suburb has yielded a rather complicated picture, and one that is difficult to summarize. The *overall* patterns revealed in the combined data for all five areas (Table 1) are in clear accord with the substantive hypotheses that gave rise to the study. More detailed analysis within individual metropolitan areas, however, revealed significant exceptions to these overall patterns. In addition, the wide variations between individual suburbs *within* both types tends to cast even more doubt upon the validity of the hypotheses as they were originally stated here. It is evident that other variables—many of them perhaps yet to be measured—have influenced growth in these larger suburbs.⁹ These results

⁹ One obvious factor requiring control is the availability of land for further residential develop-

TABLE 3. FREQUENCY DISTRIBUTION OF PERCENTAGE CHANGES IN POPULATION, BY COMPONENTS OF CHANGE,
FOR EMPLOYING AND RESIDENTIAL SUBURBS IN THE FIVE LARGEST STANDARD
METROPOLITAN AREAS, 1940-1950

Component of Change and Type of Suburb	Number of Places, by Per Cent Change, 1940-1950										Total Number of Places	
	Increase						Decrease					
	60.0 or more	30.0	15.0	10.0	5.0	0.0	0.0	5.0	10.0			
Natural increase												
Employing	1	16	24	23	2		1	1			68	
Residential	3	19	15	25	5						67	
Net migration												
Employing	1	3	10	5	4	10	17	10	8		68	
Residential	6	8	7	9	11	6	11	8	1		67	
Total change												
Employing	1	12	13	13	11	9	8	1			68	
Residential	10	12	21	6	7	8	3				67	

might suggest the need for further study in terms of more sophisticated statistical techniques. Yet, in view of some of the intrinsic deficiencies in the basic data for the period under consideration, and the limited number of cases available for analysis, it is questionable whether or not more refined manipulation is warranted.¹⁰

Despite these limitations, this study underscores the role of one quantitative factor that seems to have been underestimated in empirical investigations of suburban growth conducted so far, including those of the writer. This is the pattern of growth of the metropolitan area as a whole. In areas growing rapidly by immigration (for example, Los Angeles) patterns of suburban growth were substantially different from those observed in areas experiencing relatively limited in-migration (for example, the older metropolitan areas of the east coast). In any case, the research design employed here now appears to have been particularly deficient in failing systematically to explore the implications of overall growth for the growth of such constituent parts as suburbs. In retrospect, if there is any one research guideline that available theory should have provided, it is the idea that the metropolitan area is an integrated functional entity, no part of which may be understood without reference to the whole.

ment; the absence of mass data prohibits any measurement of the effect of this factor in studies of any substantial scope and coverage.

¹⁰ See Duncan, *op. cit.*, for an extended discussion of the serious deficiencies in the census and registration materials available for sub-parts of metropolitan areas.

THE RELATION OF INDICES OF FETAL AND INFANT LOSS TO RESIDENTIAL SEGREGATION: A FOLLOW-UP REPORT

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Some years ago, one of us (A.Y.) reported on the relationship of racially segregated neighborhoods to fetal and infant mortality.¹ Data were based upon vital statistics by New York City health areas for the years 1945-47. It was found that the rate of white and non-white fetal, neonatal and post-neonatal mortality increased as the proportion of Negro births to total births in a neighborhood increased. This finding could be related to characteristics of the neighborhoods themselves but not to identifiable characteristics (occupation or education) of Negro families residing in these neighborhoods.

Since 1947, social action has taken place—notably decisions of the United States Supreme Court, expansion of the public housing program, and anti-discrimination legislation in New York State—which might be expected to lower the rates of mortality for non-whites reported for 1945-47. To determine whether or not such an effect had occurred, the vital statistics analyses were repeated using data for the years 1953-55.

As in the previous study, vital data of pregnancy and infant wastage by health area (exclusive of the County of Richmond) were uti-

¹ A. Yankauer, "The Relationship of Fetal and Infant Mortality to Residential Segregation," *American Sociological Review*, 15 (October, 1950), pp. 644-648.

TABLE 1. DISTRIBUTION OF LIVE BIRTHS IN HEALTH AREAS GROUPED BY PERCENTAGE OF NON-WHITE LIVE BIRTHS, NEW YORK CITY (EXCLUSIVE OF RICHMOND), 1945-47 AND 1953-55

Per Cent of Non-White Births in Health Area	Number of Live Births				Per Cent of All Live Births			
	1945-47		1953-55		1945-47		1953-55	
	White	Non-White	White	Non-White	White	Non-White	White	Non-White
Less than 5	317,563	2,868	226,718	3,005	81.5	6.6	62.7	3.9
5-9	29,410	2,200	50,820	4,073	7.5	5.1	14.1	5.2
10-24	24,628	4,760	49,683	9,175	6.3	11.1	13.7	11.3
25-49	10,931	6,190	19,819	11,620	2.8	14.0	5.5	14.9
50-74	5,477	9,056	11,262	16,210	1.4	20.8	3.1	20.8
75 or more	1,703	18,420	3,256	33,708	0.4	42.4	0.9	43.4
Total	389,712	43,494	361,558	77,791	100.0	100.0	100.0	100.0

lized.² Each of 415 health areas was classified according to the percentage of its total non-white live births³ and the vital data grouped accordingly. Mortality rates were then calculated as weighted annual averages. In 1950, Negroes comprised 95.6 per cent of New York City's non-white population so that the terms non-white and Negro as used in this report are virtually synonymous. The County of Richmond was excluded from both reports.

RESULTS

During the three year period surveyed, 17.7 per cent of the live births in New York City were non-white as compared to 10.3 per cent eight years previously. The number of non-white births increased 78 per cent during these eight years while the number of white births actually decreased by 7.2 per cent. These striking figures are reflections of the migration of the non-white population into Northern central cities, the suburban emigration of the more favored and younger segments of the city's white population, and the relatively high non-white birth rate.

Furthermore, these figures are only a partial reflection of the magnitude of these movements. During 1945-47, the proportion of white births to mothers who were natives of Puerto Rico was not known. It is known, however, that between 1950 and 1955 the number of Puerto Rican births in New York City doubled and that in the years 1953-55, ten per cent of the total live births in New York City were of Puerto

Rican parentage.⁴ In the data presented in this report about 95 per cent of the Puerto Rican births are classified as white.

TABLE 2. CHANGES IN FETAL AND INFANT LOSS, NEW YORK CITY (EXCLUSIVE OF RICHMOND), 1945-47 COMPARED WITH 1953-55

	Index of Loss*		
	Fetal Mortality	Neonatal Mortality	Post-Neonatal Mortality
Rate per 1000 live births			
1945-47			
White	76.2	19.6	6.1
Non-white	135	33.7	13.8
1953-55			
White	108	16.6	5.4
Non-white	211	29.3	9.6
Per cent change in rates since 1945-47			
White	+41.9	-15.3	-11.5
Non-white	+56.4	-13.0	-30.5
Ratio of non-white to white rates			
1945-47	1.77	1.72	2.27
1953-55	1.95	1.77	1.77

* All products of gestation born without life are reportable as fetal deaths in New York City. Neonatal deaths are deaths of live-born infants prior to the 28th day of life; post-neonatal deaths are deaths of live-born infants from 28th day through the 11th month of life.

⁴ C. L. Erhardt, "Public Health Statistics in New York City on the Ninetieth Anniversary of the Board of Health," *Milbank Memorial Fund Quarterly*, 35 (January, 1957), pp. 29-47.

² Bureau of Records and Statistics, Department of Health, City of New York, *Vital Statistics by Health Areas and Health Center Districts*, 1953, 1954, 1955.

³ As in the previous study, six groupings were used: 0-4%, 5-9%, 10-24%, 25-49%, 50-74%, and 75% and over.

During the eight years between the two time periods under study a tremendous growth of New York's non-white population unquestionably occurred. This expanding population could be accommodated either by direct spread of the Negro ghetto into its surrounding fringe or by a change in the pattern of housing discrimination, permitting the Negro freer choice of dwelling in all city neighborhoods.

As illustrated in Table 1, the earlier pattern of segregation had not changed appreciably eight years later in spite of this tremendous growth. In 1945-47, 42.4 per cent of the non-white births were allocated to 24 health areas where 91.5 per cent of the total births were non-white. In 1953-55, 43.4 per cent of the non-white births were allocated to 27 health areas where 91.4 per cent of the total births were non-white.

When indices of fetal and infant loss for all races are compared for the two time periods,

it is found that fetal mortality rose while neonatal and postneonatal mortality fell. When white and non-white indices of loss are examined separately it is evident that the declines in white and non-white neonatal mortality are comparable but that the rise in fetal mortality and the decline in postneonatal mortality is greater for non-whites than for whites (Table 2). This is further illustrated in Table 2 by a comparison of the rates (ratio white: non-white rates) in the two time periods studied: in both periods the relative disadvantages of the non-white rates are impressively large.

Changes in the relationships of white and non-white rates by health areas, grouped (as described above) according to non-white birth density, are portrayed in Charts 1, 2, and 3. The charts do not show white rates for areas of highest non-white birth density and the non-white rates for areas of lowest non-white birth density because the number of births involved

CHART 1.
WHITE AND NON-WHITE FETAL DEATHS PER 1000 LIVE BIRTHS
HEALTH AREAS GROUPED BY PERCENTAGES OF NON-WHITE LIVE BIRTHS
NEW YORK CITY EXCLUSIVE OF RICHMOND
1945-47 and 1953-55

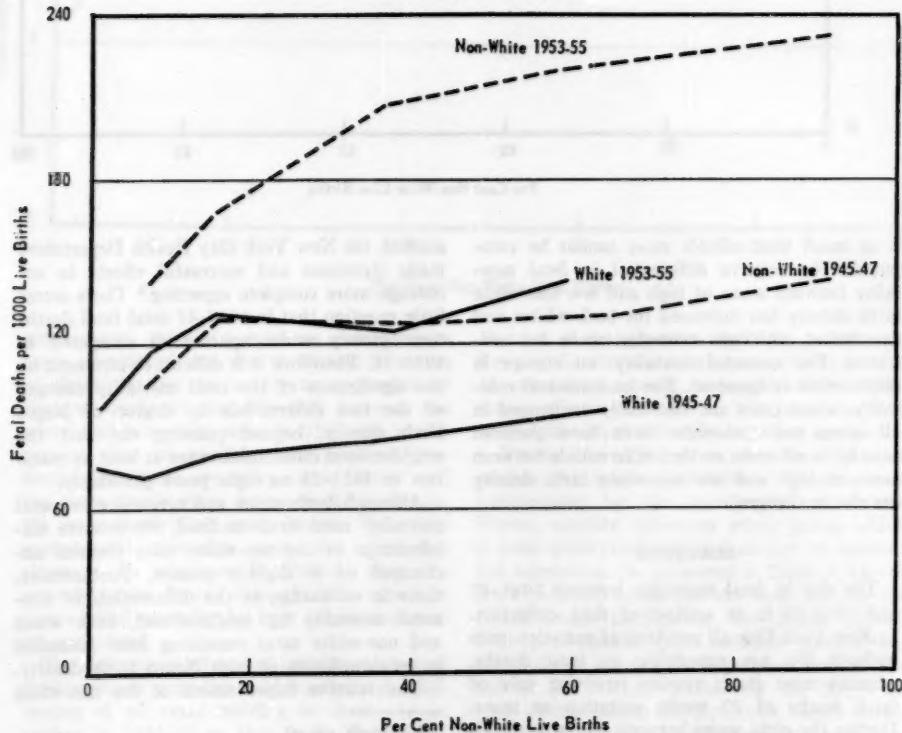
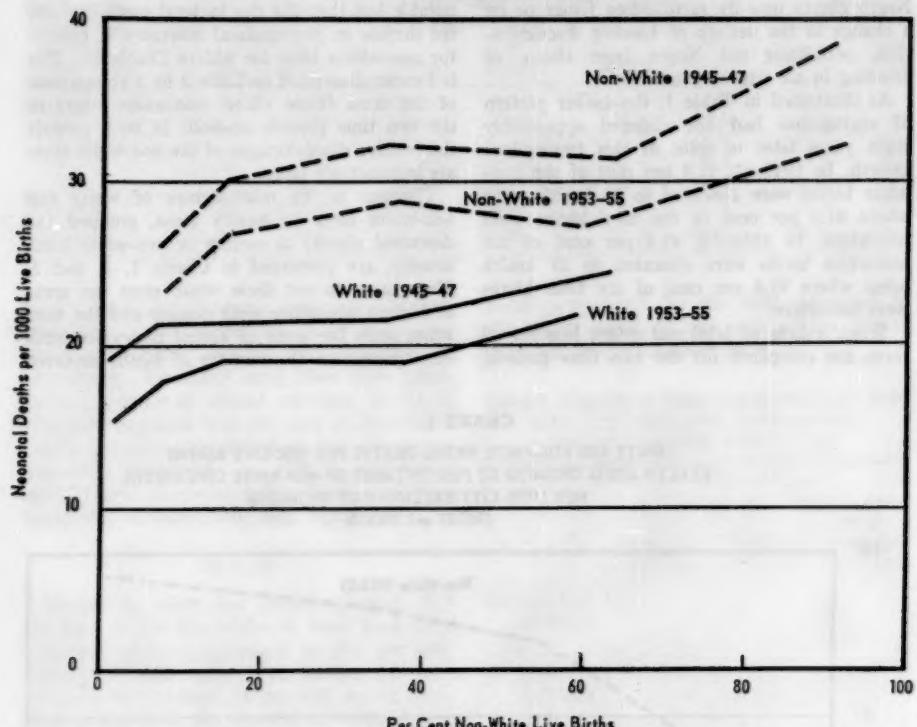


CHART 2.
WHITE AND NON-WHITE NEONATAL DEATHS PER 1000 LIVE BIRTHS
HEALTH AREAS GROUPED BY PERCENTAGES OF NON-WHITE LIVE BIRTHS
NEW YORK CITY EXCLUSIVE OF RICHMOND
1945-47 and 1953-55



is so small that reliable rates cannot be computed. The relative differential in fetal mortality between areas of high and low non-white birth density has increased for both whites and non-whites, although somewhat more for non-whites. For neonatal mortality, no change in differentials is apparent. For postneonatal mortality, white rates are essentially unchanged in all areas and non-white rates have declined equally in all areas so that differentials between areas of high and low non-white birth density are also unchanged.

DISCUSSION

The rise in fetal mortality between 1945-47 and 1953-55 is an artifact of data collection. In New York City all products of gestation born without life are reportable as fetal deaths, whereas most states require reporting only of fetal deaths of 20 weeks gestation or more. During the eight years between the two periods

studied, the New York City Health Department made strenuous and successful efforts to encourage more complete reporting.⁵ There seems little question that in 1945-47 total fetal deaths were grossly under-reported as compared to 1953-55. Therefore it is difficult to comment on the significance of the fetal mortality changes or the rate differentials by degree of Negro birth density, beyond pointing out that the neighborhood differentials were at least as manifest in 1953-55 as eight years previously.

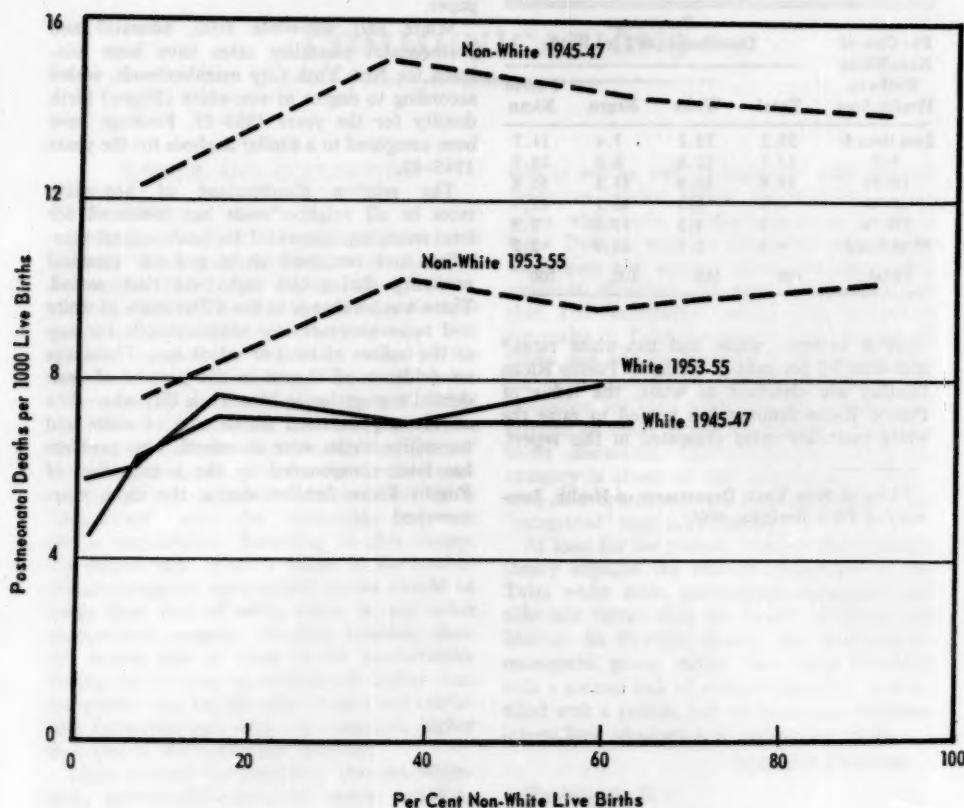
Although both white and non-white neonatal mortality rates have declined, the relative disadvantage of the non-white rates remains unchanged or is slightly greater. Furthermore, there is no change in the differentials of neonatal mortality by neighborhood, both white and non-white rates remaining least favorable in neighborhoods of high Negro birth density.

The relative improvement of the non-white

⁵ Erhardt, *op. cit.*

CHART 3.

**WHITE AND NON-WHITE POSTNEONATAL DEATHS PER 1000 LIVE BIRTHS
HEALTH AREAS GROUPED BY PERCENTAGES OF NON-WHITE LIVE BIRTHS
NEW YORK CITY EXCLUSIVE OF RICHMOND
1945-47 and 1953-55**



postneonatal mortality rates as compared to those of whites is not due to changes in causes of death. It may be merely an expression of the magnitude of the difference between the white and non-white rates in 1945-47. In the present state of scientific knowledge an irreducible minimum mortality must persist and the more closely rates approach the minimum, the more slowly they must decline. The white postneonatal mortality rate was obviously closer to its minimum in 1945-47 than the non-white rate. In any case, the differentials in postneonatal mortality by type of neighborhood are unchanged.

The deleterious effects of racially segregated neighborhoods upon the child-bearing and child-rearing of all races residing in them are as evident in 1953-55 as they were in 1945-47.

In view of the apparent continuing patterns of housing discrimination, as evidenced by a lack of substantial change in neighborhood distribution of non-white births, it is surprising that the neighborhood differentials are not greater.

During the eight years between the two studies, the housing problems of Negroes were compounded by the in-migration of Puerto Ricans, another American ethnic group which is both underprivileged and subject to residential segregation. As portrayed in Table 3, Puerto Rican families reside for the most part in the fringe areas surrounding the Negro ghetto. This phenomenon must intensify competition for available housing in fringe areas and further restrict the housing market for the Negro to areas of high Negro density. Since Puerto Rican neonatal and postneonatal mortality rates are

TABLE 3. PERCENTAGE DISTRIBUTION OF WHITE,
NEGRO AND PUERTO RICAN LIVE BIRTHS BY
HEALTH AREAS GROUPED ACCORDING TO
PROPORTION OF NON-WHITE BIRTHS
NEW YORK CITY (EXCLUSIVE OF
RICHMOND), 1956

Per Cent of Non-White Births in Health Area	Percentage Distribution of Live Birth			
	Total	White	Negro	Puerto Rican
Less than 5	52.2	72.2	7.4	14.7
5-9	12.7	11.8	6.6	26.7
10-24	14.8	10.6	15.1	35.8
25-49	6.6	3.3	13.7	12.9
50-74	5.3	1.5	17.3	7.2
75 or more	8.4	0.5	39.9	2.7
Total	100	100	100	100

midway between white and non-white rates⁶ and since 95 per cent of births to Puerto Rican families are classified as white, the influx of Puerto Rican families has tended to raise the white mortality rates presented in this report.

⁶ City of New York, Department of Health, *Summary of Vital Statistics, 1955*.

SUMMARY

In the interval 1947-1953 a number of social actions took place which might be expected to have influenced the fetal and infant mortality rates for New York City reported in an earlier paper.

White and non-white fetal, neonatal and postneonatal mortality rates have been analyzed for New York City neighborhoods, scaled according to degree of non-white (Negro) birth density for the years 1953-55. Findings have been compared to a similar analysis for the years 1945-47.

The relative disadvantage of non-white rates in all neighborhoods has increased for fetal mortality, decreased for postneonatal mortality, and remained unchanged for neonatal mortality during this eight-year time period. There was no change in the differentials of white and non-white rates by neighborhoods for any of the indices of fetal or infant loss. There was no evidence of change in the pattern of residential segregation in New York City when data on the neighborhood distribution of white and non-white births were examined. This problem has been compounded by the in-migration of Puerto Rican families during the eight years surveyed.

COMMUNICATIONS

SUICIDE AND OCCUPATION

To the Editor:

Elvin H. Powell, in "Occupation, Status, and Suicide: Toward a Redefinition of Anomie" (*American Sociological Review*, April, 1958), presents data which shows professional-managerial as the occupational category most frequently occupied by the white males of the Standard Metropolitan Area of Tulsa, Oklahoma over a period of twenty years. Jack P. Gibbs and Walter T. Martin in "A Theory of Status Integration and Its Relationship to Suicide" (in the same issue), indicate that professional-managerial is the occupational status most highly "integrated" with the white-male, color-sex status combination. According to this theory, the suicide rate of white males in the professional-managerial occupational status should be lower than that of white males in any other occupational category. The data, however, show the suicide rate of those in the professional-managerial category as consistently higher than the suicide rate for the sales-clerical and craftsmen categories and, with one exception, higher than that in the operatives category.

There remains the possibility that the white-male, professional-managerial status combina-

tion is not as well "integrated" with age, religion, or marital status, for instance, as are other white-male, occupation status combinations. Data to test this possibility with respect to religion and marital status are not readily available. However, age data are readily available. The "integration" of age with occupation for males in Tulsa computed in the manner of Gibbs and Martin is shown in the accompanying table. Nonwhite males are included in the tabulation but they represent only a very small fraction of those in the occupational categories under discussion. The professional-managerial category is about as well integrated as craftsmen, and operatives, and even somewhat more "integrated" than sales-clerical.

At least for the present, it seems that Powell's theory explains the relative magnitude of the Tulsa white male, professional-managerial suicide rate better than the theory of Gibbs and Martin. In Powell's theory, the professional-managerial group, rather than being identified with a greater lack of status integration, is identified with a greater lack of integration between internalized ideology and personality needs.

BERTHOLD BRENNER

Washington, D. C.

INTEGRATION OF OCCUPATIONAL STATUS WITH AGE FOR MALES IN TULSA COUNTY, 1950 *

Age in Years	Occupational Category					
	Pro-fessional-Managerial	Sales-Clerical	Craftsmen (Skilled Labor)	Operatives (Semi-Skilled Labor)	Unskilled Labor	Service Workers
14 to 24	.06	.20	.11	.20	.24	.18
25 to 34	.25	.30	.30	.30	.23	.19
35 to 44	.27	.20	.26	.24	.21	.18
45 to 54	.23	.16	.19	.16	.17	.20
55 to 64	.14	.10	.11	.08	.11	.17
65 and over	.05	.04	.03	.02	.04	.08
Σ X	1.00	1.00	1.00	1.00	1.00	1.00
Status integration = ΣX^2	.2140	.2072	.2188	.2200	.1972	.1762
Per cent nonwhites included	2.1	1.9	3.8	7.6	27.7	39.5

* U. S. Department of Commerce, *Seventeenth Census of the United States: 1950 Population*, Vol. 11, Part 36, Washington: U. S. Government Printing Office, 1952, pp. 200-201, 206-207.

TERMINOLOGICAL PROBLEMS

To the Editor:

In the April, 1958 issue of the *Review* Dr. Panos D. Bardis takes me to task for the special sense in which I made use of the term "dialogue." (p. 202) In view of the fact that I have never since used the term in this sense, I might plead that an error not repeated could be excused by any well-wisher. I do not make such a plea, however, for I do not regard the usage as erroneous. Quoting from a recognized handbook, H. W. Fowler's *A Dictionary of Modern English Usage* (1st edition, London: Humphrey Milford, 1926):

dialogue is neither necessarily, nor necessarily not, the talk of two persons; . . . for the want of a word confined to two, see *DUOLOGUE*. (p. 112).

duologue is a bad formation, but there are difficulties in the way of making a good one; *dyologue*, which is better only in one respect, is indistinguishable in sound from *dialogue*; *dilogy* conflicts with *trilogy & tetralogy*; *dittologue* suggests *ditto*; *biloquy* after *soliloquy* is less bad than *duologue* after *monologue*. The best course is to get along as well as may be with *dialogue* . . . & periphrasis; barbarous formation is peculiarly bad in words that are designed only for the educated. (p. 124).

Let me call attention to one key phrase: "the best course is to get along as well as may be with dialogue . . . & periphrasis. . ." It seems to me, therefore, that my use of "dialogue" in the sense of conversation between only two persons is quite permissible, regardless of etymology.

Now, with regard to technical terms in general, I should like again to offer a quotation, this time from H. W. Fowler and F. G. Fowler's *The King's English* (3rd edition: Oxford: Clarendon Press, 1931, p. 29): ". . . it might indeed be desired that the man of science should always call in the man of Greek composition as god-father to his *gas* or his *process*; but it is a point of less importance. Every one has been told at school how *telegram* ought to be *telepheme*;

but by this time we have long ceased to mourn for the extra syllable. . . ."

Dr. Bardis is doubtless quite familiar with Greek and possibly with Latin, but this familiarity may have betrayed him into undue reliance on etymology parallel to the pedantic insistence on *telepheme* instead of *telegram*. In scientific terminology, and even in ordinary English, reliance on etymology is dangerous in the extreme, as is shown by the fact that a "check dress," meaning thereby a dress made of cloth having a check, checked, or checkered pattern, might lead Dr. Bardis to the inference that the dress had been given to the woman wearing it by the Shah of Persia. After all, the term "check" comes from "Shah," as in our term "checkmate," deriving from *Shah mat*, "The King is dead." From the game of chess, and its relative, checkers, came the reference to the board on which the game is played, and from this in turn to the pattern itself. (Here see *Shorter Oxford English Dictionary*, vol. 1, entry "check," p. 297.) It might be well, therefore, to pay a little less attention to etymology, and a little more to current usage.

The remark has also been made by Dr. Bardis that "trialogue" does not occur in any language. Here he might refer to Leonard Bloomfield, *Language* (New York: Holt, 1933, pp. 255, 257), where mention is made of *trial* number (three persons). Some Melanesian languages are characterized by the extensive use of trial forms. Ergo, *trialogue*.

Where "polylogue" is concerned, again let me make reference to the dangers of simple etymological derivation. In connection with dialogue and trialogue, it would seem clear that "polylogue" makes contemporary sense, regardless of what it meant in ancient Greek.

This entire matter, from start to the present rejoinder, might justly be viewed as a tempest in a teapot—but, "Sow the wind and reap the whirlwind."

HOWARD BECKER

University of Wisconsin

Grants for Asian Sociologists

The American Sociological Society has received from The Asia Foundation a grant of \$2,500, for the purpose of encouraging closer relations between Asian and American sociologists. The funds will be used in three ways:

- (1) To enable Asian sociologists to become members of the American Sociological Society and to receive a three-year subscription to one or more of its official publications.

(Membership in the Society and a three-year subscription to the *American Sociological Review* will be \$1.00; if all Society publications are desired, the three-year cost will be \$2.00. Applicants should write directly to The American Sociological Society, New York University, Washington Square, New York 3, New York. Payment may be made in UNESCO coupons or in any way convenient and acceptable under the exchange regulations of the Asian country concerned. The privilege is extended to graduate students as well as to established sociologists.)

- (2) To enable libraries, university departments, and research institutes in Asia, who have heretofore been unable to subscribe, to subscribe to publications of the Society at reduced rates.

(The cost of a three-year institutional subscription to the *American Sociological Review* will be \$2.00; and for all the publications of the Society, including *Sociometry*, \$3.00—payable as above.)

- (3) To supplement travel expenses for Asian sociologists who are in the United States and who wish to attend meetings of the American Sociological Society.

(Applicants must be at least at the graduate student level and may come from any Asian country from Afghanistan eastward. An applicant should write to the Chairman of the administering committee, as listed below. In his request the applicant should give his regular academic position, the nature of his study or visit in the United States, the meeting which he plans to attend, and the sum necessary for transportation to and from the meeting.)

The grant is being administered by a special committee composed of the following:

Professor Kingsley Davis, *Chairman*, Department of Sociology and Social Institutions, University of California, Berkeley, California.

Professor Wolfram Eberhard, Department of Sociology and Social Institutions, University of California, Berkeley, California.

Professor Amos H. Hawley, Department of Sociology, University of Michigan, Ann Arbor, Michigan.

Professor Marion J. Levy, Jr., Department of Sociology, Princeton University, Princeton, New Jersey.

Professor Bryce F. Ryan, Department of Sociology and Anthropology, University of Miami, Coral Gables 46, Florida.

THE PROFESSION: REPORTS AND OPINION

THE CONCEPTS OF CULTURE AND OF SOCIAL SYSTEM

A. L. KROEGER

Berkeley, California

AND

TALCOTT PARSONS

Harvard University

There seems to have been a good deal of confusion among anthropologists and sociologists about the concepts of *culture* and *society* (or, *social system*). A lack of consensus—between and within disciplines—has made for semantic confusion as to what data are subsumed under these terms; but, more important, the lack has impeded theoretical advance as to their interrelation.

There are still some anthropologists and sociologists who do not even consider the distinction necessary on the ground that all phenomena of human behavior are sociocultural, with both societal and cultural aspects at the same time. But even where they recognize the distinction, which can be said now to be a commonplace, they tend to assume determinative primacy for the set of phenomena in which they are more interested. Sociologists tend to see all cultural systems as a sort of outgrowth or spontaneous development, derivative from social systems. Anthropologists are more given to being holistic and therefore often begin with total systems of culture and then proceed to subsume social structure as merely a part of culture. ("Social anthropology" perhaps represents a secession within anthropology that inclines to prefer the sociological assumption.)

Our objective in the present joint statement is to point out, so far as methodological primacy is concerned, that, either of these assumptions is a preferential *a priori* and cannot be validated in today's state of knowledge. Separating cultural from societal aspects is not a classifying of concrete and empirically discrete sets of phenomena. They are distinct systems in that they abstract or select two analytically distinct sets

of components from the same concrete phenomena. Statements made about relationships within a cultural pattern are thus of a different order from those within a system of societal relationships. Neither can be directly reduced to terms of the other; that is to say, the order of relationships within one is independent from that in the other. Careful attention to this independence greatly increases the power of analytical precision. In sum, we feel that the analytical discrimination should be consistently maintained without prejudice to the question of which is more "important," "correct" or "fundamental," if indeed such questions turn out to be meaningful at all.

It is possible to trace historically two successive analytical distinctions that have increased this analytical precision. It might be suggested that the first differentiation was a division of subject-matter broadly along the lines of the heredity-environment distinction. In English-speaking countries, at least, the most important reference point is the biologically oriented thinking of the generation following the publication of Darwin's *Origin of Species*. Here the social scientists were concerned with defining a sphere of investigation that could not be treated as simply biological in the then current meaning of that concept. Tylor's concept of culture and Spencer's of the social as superorganic were important attempts to formulate such a sphere. Thus the organism was assigned to the biological sciences and culture-society (as yet more or less undifferentiated) assigned to the sociocultural sciences.

In the formative period of both disciplines, then, culture and society were used with relatively little difference of meaning in most works of major influence. In the anthropological tradition, Tylor and Boas used culture to designate that aspect of total human social behavior (including its symbolic and meaningful products) that was independent of the genetic constitutions and biological characteristics of organisms. The ideas of continuity, creation, accumulation, and transmission of culture independent of biological heredity were the key ones. On the soci-

ological side, Comte and Spencer, and Weber and Durkheim spoke of society as meaning essentially the same thing that Tylor meant by culture.

For a considerable period this condensed concept of culture-and-society was maintained, with differentiation between anthropology and sociology being carried out not conceptually but operationally. Anthropologists tended to confine their studies to nonliterate societies and sociologists concerned themselves with literate ones (especially their own.) It did not seem necessary to go much further. Now we believe that knowledge and interests have become sufficiently differentiated so that further distinctions need to be made and stabilized in the routine usage of the relevant professional groups. Such a need has been foreshadowed in the practice of many anthropologists in speaking of social organization as one major segment or branch of culture, and of some sociologists in discriminating such categories as values, ideologies, science, and art from social structure.

In this way a second analytical distinction has taken (or is taking) shape. We suggest that it is useful to define the concept *culture* for most usages more narrowly than has been generally the case in the American anthropological tradition, restricting its reference to transmitted and created content and patterns of values, ideas, and other symbolic-meaningful systems as factors in the shaping of human behavior and the artifacts produced through behavior. On the other hand, we suggest that the term *society*—or more generally, *social system*—be used to designate the specifically relational system of interaction among individuals and collectivities. To speak of a "member of a culture" should be understood as an ellipsis meaning a "member of the society of culture X." One indication of the independence of the two is the existence of highly organized insect societies with at best a minimal rudimentary component of culture in our present narrower sense.

Parenthetically we may note that a similar analytical distinction has begun to emerge with reference to the older concept of the organism, on the other side of the division outlined above by which the social sciences came to be differentiated from the biological. Where the term organism was once used to designate both biological and psychological aspects, it has recently come to be increasingly important to discriminate a specifically psychological component from the merely biological. Thus the term personality is being widely used as an appropriate or favored term expressive of the distinction.

To speak, then, of the analytical independence

between culture and social system is, of course, not to say that the two systems are not related, or that various approaches to the analysis of the relationship may not be used. It is often profitable to hold constant either cultural or societal aspects of the same concrete phenomena while addressing attention to the other. Provided that the analytical distinction between them is maintained, it is therefore idle to quarrel over the rightness of either approach. Important work has been prosecuted under both of them. It will undoubtedly be most profitable to develop both lines of thinking and to judge them by how much each increases understanding. Secondly, however, building on the more precise knowledge thus gained, we may in time expect to learn in which area each type of conceptualization is the more applicable and productive. By some such procedure, we should improve our position for increasing understanding of the relations between the two, so that we will not have to hold either constant when it is more fruitful not to do so.

We therefore propose a truce to quarreling over whether culture is best understood from the perspective of society or society from that of culture. As in the famous case of heredity "versus" environment, it is no longer a question of how important each is, but of how each *works* and how they are interwoven with each other. The traditional perspectives of anthropology and sociology should merge into a temporary condominium leading to a differentiated but ultimately collaborative attack on problems in intermediate areas with which both are concerned.

OCCUPATIONAL ROLES OF SOCIOLOGISTS

RAYMOND V. BOWERS

*Air Force Personnel and Training Research
Center, United States Air Force*

In February of 1958, all 2,500 active members of the American Sociological Society were sent a questionnaire requesting information about their jobs. The purpose of the survey was to provide current data for the Society's brochure on the roles of sociologists, which is being prepared by an Editorial Board appointed by President Robin Williams.

Nearly half of the questionnaires were returned by April 1, when the flow had practically stopped. Preliminary hand counts show the or-

ganizational contexts of the "main job" listed by these 1,200 sociologists to be as follows:

Education	78%
Government	8
Business	5
Social Welfare	2
Medical, Legal, and Religious Organizations	2
Foundations	2
Other (mainly retired sociologists)	3

Total	100%
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A further breakdown of the academic group shows that they listed various types of appointments:

Sociology department only	63%
Other department only	6
Research appointment, full-time or combined with teaching	6
Administration appointment (other than departmental chairmanship)	3

Total	78%
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Thus sociology is still primarily an academic profession, although only 63 per cent of our members who responded are in the traditional role of "members of sociology departments." Another 15 per cent are in academic life but have part- or full-time duty outside a sociology department. Another 22 per cent work outside academic institutions.

Each member was also asked to indicate the "functions" of his job by checking one or more of a set of choices. The results are as follows (a person could check as many functions as were pertinent to his job):

Functions of Job	Organizational Context of Job		
	Education	Government	Business
Teaching	95%	22%	5%
Research	70	82	54
Administration	32	60	32
Practice	5	15	18
Other	22	37	10
Number of Respondents	(936)	(95)	(60)

Thus the research function has become a close second to teaching in the proportion of academic sociologists claiming it as a part of their jobs, and it is the primary function for sociologists outside of academic life. Administrative work is the third most important function in the jobs held by academic sociologists, while it is second for those in government and business.

In order to supplement and enhance these statistics, the Editorial Board for the brochure is also collecting career descriptions from a sample of the Society's members.

OFFICIAL REPORTS AND PROCEEDINGS

NOTICE CONCERNING THE 1959 ANNUAL MEETING

The 1959 Annual Meeting of the Society will be held at the Edgewater Beach Hotel, Chicago, Illinois, September 3, 4 and 5. Joint sessions are being arranged with the American Anthropological Association, the Rural Sociological Society and the Society for the Study of Social Problems. The following sections and chairmen have been arranged by President Kingsley Davis and the 1959 Program Committee:

Sections and Chairmen

Sociological Theory
Lewis A. Coser, University of California,
Berkeley

Methodology
Herbert Hyman, Columbia University

Social Psychology
Guy E. Swanson, University of Michigan

Political Sociology
Robert E. Lane, Yale University

Industrial Sociology
Robert Dubin, University of Oregon

Criminology
Donald R. Cressey, University of California, Los Angeles

Sociology of Science
Francis X. Sutton, Ford Foundation

Sociology of the Arts and Popular Culture
Herbert A. Bloch, Brooklyn College

Race and Ethnic Relations
William O. Brown, Boston University

The Family and Marriage
William M. Kephart, University of Pennsylvania

Population
Irene Taeuber, 4222 Sheridan Street, Hyattsville, Maryland

- Sociology of Education*
Martin A. Trow, University of California, Berkeley
- Sociology of Religion*
Gerhard E. Lenski, University of Michigan
- Social Stratification*
A. B. Hollingshead, Yale University
- Urban Sociology*
Gideon Sjoberg, University of Texas
- Sociology of Organizations*
Philip Selznick, University of California, Berkeley
- Sociology of Small Groups*
Omar Khayyam Moore, Yale University
- International Sociology*
Marion J. Levy, Jr., Princeton University
- Sociology of Law*
F. James Davis, Hamline University
- Communication and Public Opinion*
Otto N. Larsen, University of Washington
- Graduate Training in Sociology*
Raymond V. Bowers, University of Georgia
- Social Disorganization and Deviant Behavior*
Austin L. Porterfield, Texas Christian University
- Social Aspects of Mental Health*
To be announced.
- Occupational Sociology*
Edward Gross, State College of Washington
- Rural Sociology*
Harold F. Kaufman, Mississippi State College
- Age Status and Social Structure*
Glen V. Fuguit, University of Wisconsin
- Social Change and Economic Development*
W. Fred Cottrell, Miami University
- Medical Sociology*
Everett C. Hughes, University of Chicago
- Social Structure and Personality*
Orville G. Brim, Jr., Russell Sage Foundation
- Rapidly Expanding Fields of Applied Sociology: Critical Analyses by Outsiders*
Donald R. Young, Russell Sage Foundation
- Problems in the Advancement of the Sociological Profession*
Edgar F. Borgatta, Russell Sage Foundation
- History and Sociology*
To be announced.

Members may submit papers directly to the chairmen or, if they are uncertain about the appropriate chairman, to the Program Committee, in care of Beverly Duncan, Department of Sociology, University of Chicago, Chicago, Illinois. Papers should not exceed 1500 words in length and must be received by February 1, 1959, at the latest.

In order to provide our growing membership with the widest opportunity to participate in the annual program, each person is allowed to read only one paper if he is the sole author, and to contribute to two programs only if he is joint author in each case or is a chairman or discussant in one case.

The sessions planned by our Program Committee promise to be original, challenging and informative. A special effort is being made at this meeting to improve the process of communication in the sessions. Also, a feature of the Chicago conference is that certain sessions will be devoted to sociology as a profession and to the advancement of the interests and position of sociologists.

NEWS AND ANNOUNCEMENTS

ELSE FRENKEL-BRUNSWIK

1908-1958

With the death of Else Frenkel-Brunswik on March 31, 1958, sociology and the other social sciences lost one of the most productive and imaginative social theorists of our time. Her fields of interest, securely based on continually enlarging first-rate scholarship,

embraced a good number of subjects, among them the methodology of the social sciences, the relationship of psychology and sociology in political behavior, the theory of human development and growth, the methodology of psychoanalysis, and the place of value concepts in social theory.

Born in Lemberg (which at the time of her birth was part of Austria), she received her

Ph.D. in psychology from the University of Vienna in 1930. From 1931 to 1938 she was a lecturer and research associate at the Psychological Institute of the University of Vienna. She came to the United States after Hitler's invasion of Austria in 1938 and, as in the case of her husband, the late Professor Egon Brunswik, accepted a post at the University of California. At Berkeley she was a professor in the Department of Psychology. During the academic year 1942-43 she was a fellow of the Social Science Research Council; in 1950 she was invited as a lecturer and research consultant by the Institute of Social Research at the University of Oslo; in 1954-55 she was a fellow at the Center for Advanced Study in the Behavioral Sciences; and at various times she was elected to offices of a number of learned societies. At the time of her death she was research associate of the Institute of Child Welfare and of the Institute of Industrial Relations, as well as research psychologist and psychotherapist at Cowell Memorial Hospital—all affiliations within the University of California.

Her scholarly productivity bordered on the prodigious. Her publications list comprises more than fifty books, monographs, and lengthy contributions to scholarly magazines, many of which have been frequently reprinted and are considered classical pieces in her fields of interest and specialization. Perhaps best known is her contribution to *The Authoritarian Personality* (1950) of which she was co-author; she pursued the findings of this study in extensive specialized research, much of which has been reported in scholarly journals and before learned symposia. Her latest publications include "Perspectives in Psychoanalytical Theory" (in *Perspectives in Personality Theory* [1957]) and "Social Research and the Problem of Values" (in the *Journal of Abnormal and Social Psychology* [1954]).

As a scholar and a personality of exquisite character and generosity, Else Frenkel-Brunswik commanded great respect and affection among her colleagues and students at Berkeley and in the world of social science in this country and in Europe. Her competence, productivity, and boundless intellectual curiosity made her a true member of

the *universitas*—aspired to by many but achieved by few.

LEO LOWENTHAL
University of California, Berkeley

The Inquiry Association, a Scandinavian organization for the promotion of debate and research on problems common to philosophy and the various branches of the behavioral and social sciences, has published the first issue of a new quarterly journal, *Inquiry*. Included on the Advisory Board are Rudolf Carnap, Campbell Crockett, Abraham Kaplan, Harold D. Lasswell, Charles W. Morris, Robert K. Merton, and David Rynin. Manuscripts should be sent to Arne Naess, Institute of Philosophy and the History of Ideas, Oscarsgt. 30, Oslo, Norway.

International Conference of Social Work and the **International Association of Schools of Social Work** are jointly publishing a new quarterly journal, *International Social Work*, the first issue of which appeared in January, 1958.

International Sociological Association. The Fourth World Congress of Sociology will be held in Perugia, Italy, in September 1959, probably from the eighth to the fifteenth. There are to be three sections: I. Sociology in Its Social Context, II. The Application of Sociological Knowledge, III. Developments in Sociological Method. All papers are by invitation in the first section; the third section is broken down into ten seminars, each chairman of which will prepare a paper. In Section II, papers may be submitted to the following chairmen of the sub-sections: (a) Industry: Professor René Clémens, University of Liège, Institute of Sociology, Belgium; (b) Agriculture: Professor E. W. Hofstee, Agricultural University, Wageningen, Netherlands; (c) Education: Mrs. Jean Floud, Institute of Education, University of London; (d) Regional and Town Planning: Mrs. Ruth Glass, Social Research Unit, University of London; (e) Public Health: Professor Gratchenkov, Academy of Sciences of the USSR, Moscow; (f) Mass Communications: Professor Morris Janowitz, University of Michigan, Ann Arbor, Michigan; (g) Population: Professor Livio Livi, Via Baldesi 18, Florence, Italy; (h) Public Administration and Organization: Mr. Henning Friis, Ministry of Labor and Social Affairs, Copenhagen; (i) Problems of Economic Growth in Underdeveloped Countries: Dr. Angel Palerm, Organization of the American States, Washington, D. C.; (j) Ethnic and Racial Relations: Professor E. Franklin Frazier, Howard University, Washington, D. C.; (k) The Family: Professor Helmut Schelsky, University of Hamburg, Germany; (l) Leisure: Dr. Ricardo Bauer, Society Umanitaria, Milan, Italy; (m) Sociology of Medicine: Dr. George Reader, New York Hospital, 525 East 68th Street, New York 21, New York.

The Social Science Research Council is administering a grant from the Carnegie Corporation of New

York for travel grants to international meetings. The committee in charge seeks persons who by their presence, participation, or organizing services, will make major contributions to the success of a conference; and scholars who by their attendance will gain ideas and contacts that will advance their own research. Special consideration is given to younger social scientists and to those who have had little opportunity to become acquainted with foreign colleagues. Application forms may be obtained from the Social Science Research Council, 230 Park Avenue, New York 17, New York. (See Social Science Research Council below.)

World Federation for Mental Health. Plans for a World Mental Health Year in 1960 sponsored by 108 mental health and professional societies in 43 countries and territories have been announced. Following the pattern of the International Geophysical Year, the purpose of the program is to stimulate mental health activities, including research, with a maximum of international cooperation.

American Institute for Research and the University of Pittsburgh are developing plans for a program of research which would help fill a long-standing need for factual information about the nature of human talents and how best to assist individuals to identify, develop, and use them. The proposal is to measure and describe a one-twentieth stratified sample of 500,000 students currently in grades 9 through 12 and follow them for twenty years. John T. Dailey has accepted the position of Program Director for the study. Those who have accepted membership on the Manpower and Sociology advisory panel are E. Franklin Frazier, Donald G. Marquis, Robert K. Merton, C. Joseph Nuesse, Fred L. Strodtbeck, and Samuel A. Stouffer, Chairman.

Illinois Institute of Technology. Creative trends in urban building will be the theme of the fourth annual National Construction Industry Conference to be held in the Sherman Hotel, Chicago, on December 10 and 11. Inquiries concerning the conference should be addressed to R. T. Mijanovich, Armour Research Foundation, 10 W. 35 Street, Chicago 16, Illinois.

Missouri Sociological Society. Officers of the Society for the current year are: Robert L. McNamara, University of Missouri, President; David B. Carpenter, Washington University, Vice-President; Wayne Wheeler, Park College, Secretary-Treasurer. The Society will hold its annual meeting in Columbia on October 18, 1958. A portion of the program will be a joint session with the Conference on Asian Affairs.

The Ohio Valley Sociological Society. The following were elected officers for 1958-59 at the 20th Annual Meeting: President, Clifford Kirkpatrick, Indiana University; Vice-President, Howard Rowland, University of Pittsburgh; Secretary-Treasurer, Marvin B. Sussman, Western Reserve University; Representative of the Society to the

Council of the American Sociological Society, Raymond F. Sletto, The Ohio State University; Editor of the *Ohio Valley Sociologist*, Robert P. Bullock, The Ohio State University.

National Science Foundation. Harry Alpert resigned on August 1 as Program Director of Social Science Research in order to accept an appointment as Dean of the Graduate School and Professor of Sociology at the University of Oregon. Henry W. Riecken, formerly of the Department of Sociology, University of Minnesota, has assumed the duties of the Program Director.

Among 246 recent recipients of grants in support of science are Richard C. Atkinson, Department of Psychology, University of California, Daniel Lerner, Center for International Studies, Massachusetts Institute of Technology, and Robert B. Zajonc, Research Center for Group Dynamics, University of Michigan.

Social Science Research Council. During the academic year 1958-1959 the Council will accept applications from permanent residents of the United States and Canada for the following types, among others, of fellowships and grants for training or research:

Predoctoral and postdoctoral Research Training Fellowships, to provide more advanced research training than that afforded by the usual Ph.D. program. Predoctoral fellowships are available only after fulfillment of all requirements for the doctoral degree with the exception of the dissertation, but applications may be submitted in anticipation of fulfilling these requirements within the ensuing year.

Grants-in-Aid of Research and Faculty Research Grants, in amounts up to 6,000 dollars, to defray direct costs or to provide free time for individual research, or both; available only to scholars who are no longer candidates for degrees and whose capacity for effective research has been demonstrated by previous work. In 1958-59, for the first time, these grants will be awarded both in January and in April, with closing dates for applications November 1 and February 1.

Special grants for social science research in the following fields: American Governmental Processes, The Near and Middle East, and Slavic and East European Studies.

International Conference Travel Grants equivalent to tourist-class fare for social scientists attending certain international meetings to be designated by the Council.

In addition to the foregoing appointments for which individuals may apply directly, nominations are invited for the following:

Faculty Research Fellowships, providing part-time release from other duties for independent research for a term of three years, for a few social science teachers, normally not over 35 years of age; available only to college and university faculty members in the United States.

Senior Research Awards in American Governmental Affairs, to provide maintenance and research expenses for one year for about five distinguished scholars.

Auxiliary Research Awards of 4,000 dollars to be awarded without reference to specific research projects to about 25 younger social scientists selected on the basis of past achievement and future promise of significant research.

Closing dates for applications or nominations will differ for the several programs, the earliest being October 15. Prospective candidates are urged to write as early as possible to the Social Science Research Council, 230 Park Avenue, New York 17. Requests for application forms should indicate age, highest academic degree held, present position or activity, and the purpose for which a fellowship or grant is desired.

The Society for the Study of Social Problems. The official journal of the Society, *Social Problems*, is planning a special issue on law and social problems. Students in the area are invited to submit papers for this issue. Manuscripts should be sent to Erwin O. Smigel, Editor, Indiana University, Bloomington, Indiana, before April 1, 1959.

Alfred University. Luke Mader Smith, Assistant Professor of Sociology at the University of Buffalo, has been appointed Chairman of the Department, succeeding Roland L. Warren who has left to become Director of the Community Study Service for the State Charities Aid Association.

Brown University. Vincent H. Whitney has returned to the Department after a leave of absence during which he served as Associate Demographic Director of the Population Council.

Harold W. Pfautz is on sabbatical leave for the fall semester.

Kurt B. Mayer and Sidney Goldstein have received a research grant from the Small Business Administration for a two-year field study of problems of business growth in Rhode Island. Their monograph, *Migration and Economic Development in Rhode Island*, has just been published. Goldstein's book, *Patterns of Mobility, 1910-1950: A Method for Measuring Migration and Occupational Mobility in the Community*, will be published early this fall.

Louis H. Giddings, Director of the Haffenreffer Museum of the American Indian, was appointed Secretary of Section H (Anthropology) of the American Association for the Advancement of Science. In May he took part in the Circumpolar Conference held in Copenhagen, and spent the summer months in Alaska, continuing his archaeological excavations.

Surinder K. Mehta spent the summer months at the Population Research and Training Center of The University of Chicago completing his research on the urban labor force of Burma.

University of Buffalo. Llewellyn Gross, Professor of Sociology, was elected President of the Upstate New York Sociological Society at the annual meeting held last May.

University of California, Berkeley. Reinhard Bendix is serving as Chairman of the Department of Sociology and Social Institutions for a three-year term, replacing Herbert Blumer. Bendix and Sey-

mour M. Lipset have recently published an evaluation and annotated bibliography of the field of Political Sociology as a special issue of *Current Sociology*, and have completed a book, *Social Mobility in Industrial Society*, to be published soon.

Herbert Blumer is on sabbatical leave in Brazil as a UNESCO research consultant to the Centro Latino-American de Pesquisas em Ciencias Sociais.

Kingsley Davis has been reappointed as the United States delegate on the Population Commission of the United Nations. Richard L. Forstall, formerly with Rand McNally and Company, joined the staff of International Urban Research last January. Erving Goffman and David Schneider (Department of Anthropology) are members of the Center for the Integration of Social Science Theory. William Kornhauser has been appointed a research associate of the Institute of Industrial Relations.

During the past summer Wolfram Eberhard taught in Rangoon, and Herbert Franz Schurmann was in Hong Kong interviewing refugees from Communist China as part of his study of Communist organizational methods and response.

New Assistant Professors are Bernard Cohen and Neil Smelser, both formerly at Harvard University. Bennett Berger and Jack Gibbs have been appointed Lecturers for the current year.

Three new appointments outside the department include Marjorie Fiske, formerly Associate Director of the Bureau of Applied Social Research of Columbia University, as Director of social science research at the Langley-Porter Clinic; Burton Clark, Jr., formerly at the Harvard School of Education, in the School of Education; and Philip Rieff, formerly at Brandeis University and Fellow at the Center for Advanced Study in Behavioral Sciences, as Assistant Professor in the Department of Speech. Among others who hold appointments outside the Department are: Jack London and Martin Trow in Education, Donald Foley in City and Regional Planning, Ernest Greenwood in Social Welfare, Judson Landis and Clark Vincent, Jr., in Home Economics (as Family Sociologists), Leo Schnore in Biostatistics in the School of Public Health, and Leo Lowenthal in Speech. Lowenthal, Schnore, and Trow are also members of the Department of Sociology.

Holding Faculty Fellowships during 1958-59 are Kunio Odaka, Professor of Sociology at Tokyo University, G. N. Ostergaard, Lecturer at the University of Birmingham, and David Lockwood, Lecturer at the London School of Economics.

Duke University. Alan C. Kerckhoff, formerly at Vanderbilt University, and Joel Smith, formerly at Michigan State University, have joined the staff as Associate Professors.

David Shaw, from the University of Alabama, and Ida Harper Simpson, formerly at University of Illinois, have been appointed Research Associates.

John C. McKinney was a Fellow at the first Summer Institute in Gerontology held at the University of Connecticut.

Howard E. Jensen has retired from active teaching after twenty-seven years in the Department.

University of Idaho. Harry C. Harmsworth, Chairman and Professor of Sociology, attended the Summer Institute in Social Gerontology at the University of Connecticut; Alfred W. Bowers, Associate Professor, directed archeological research at the Ryghs Site, near Mobridge, South Dakota; and Myra S. Minnis, Assistant Professor, conducted a summer tour of European communities.

University of Illinois. The program for the Ph.D. in Communications has recently been revised. For information, please address Dallas W. Smythe, Chairman, Committee on Graduate Study in Communications, 119 Gregory Hall, University of Illinois, Urbana.

University of Kansas. Robert C. Squier and Charles A. Valentine, from Berkeley and the University of Pennsylvania respectively, will join the staff in September as assistant professors in anthropology. Leaving the department this year are Rupert I. Murrill, who becomes Assistant Professor at University of Minnesota, John T. Gullahorn, who becomes Assistant Professor at Michigan State University, and Louise Sweet, who has accepted a Social Science Research Council grant to study Near-Eastern cultural life. Marston M. McCluggage has returned from the University of Washington, Seattle. Lawrence S. Bee's *Marriage and Family Relations* is now in press. E. Gordon Erickson, who is completing *The Population of the West Indies Federation*, was Visiting Professor at Washington University, St. Louis, last summer. Charles K. Wariner, recipient of a Fulbright research grant, is conducting research in the Philippines.

The University of Kansas City. Ernest Manheim is now Henry Haskell Professor of Sociology. Leon Warshay has joined the Department as an Instructor.

Los Angeles State College. The title of the Department has been changed to the "Department of Sociology and Anthropology." Gilbert Geis attended the Summer Research Training Institute in the Judicial Process at the University of Wisconsin on a Fellowship from the Social Science Research Council.

Louisiana State University. Homer L. Hitt has resigned as Head of the Departments of Sociology and Rural Sociology, and is succeeded by Roland J. Pellegrin, and as Associate Dean of the Graduate School, to become Dean of Louisiana State University at New Orleans.

Alvin L. Bertrand, who was on leave of absence to serve as Head, Levels of Living Section, Farm Population and Rural Life Branch, Agricultural Marketing Service, U. S. Department of Agriculture, has returned to the University.

Paul H. Price has been promoted to Professor of Sociology and Rural Sociology.

Frederick L. Bates has joined the faculty as Visiting Associate Professor of Sociology and Rural Sociology.

Walfrid J. Jokinen has been appointed Assistant

Dean of the Graduate School and Assistant Professor of Sociology.

Aubrey F. Borenstein has been appointed Instructor in Sociology.

University of Missouri, Department of Rural Sociology. Robert L. McNamara is president of the Missouri Sociological Society and president-elect of the Missouri Public Health Association. Herbert F. Lionberger has been promoted to Professor and Cecil L. Gregory to Assistant Professor.

University of Pennsylvania. Thorsten Sellin was recently a member and rapporteur of an international committee which met at the United Nations to plan the future work program in criminology and the next World Congress (1960) of the UN on the prevention of crime and the treatment of offenders.

James H. S. Bossard and Eleanor Stoker Boll are co-authors of *Why Marriages Go Wrong*; the authors' comparative study, *Families By Size*, is scheduled for publication in 1960.

Otto Pollak is directing a research project at the Family Service of Philadelphia designed to measure differences in the effectiveness of two types of case work treatment.

Fellowships from the Samuel S. Fels and George Leib Harrison Funds will permit Allen D. Grimshaw to devote full time to his research on the sociology of violence.

Three members of the Department are abroad: Everett Lee at the University of Kiel, E. Digby Baltzell in Spain, and Norman Johnston in Spain and England. Marvin Wolfgang has returned from a year in Italy.

Albert H. Hobbs, William M. Kephart, and Richard D. Lambert have been promoted to the rank of Associate Professor. Marvin Bressler accepted a position as Associate Professor at New York University. Donald McKinley is an Instructor in the Department, and Harry Bash, Albert Chevan, Stephen Fybish, and John L. Thompson are serving as teaching assistants.

Saint Louis University. The Department, which continues under the direction of Clement S. Mihovich, Professor of Sociology, has added an Anthropology program. The new program includes Allen Spitzer, Associate Professor of Anthropology and Director of Anthropological Research, and Joan De Pena (Indiana University), Instructor in Anthropology. Spitzer has published field work on folk Catholicism and is continuing work on the Montana Blackfeet on a grant from the Human Relations Center. He has received permanent appointments as Research Professor of Anthropology at Mexico City College and at the University of Yucatan. Formerly president of the American Catholic Sociological Society, he serves on the executive board of the Catholic Anthropological Conference, and as advisory editor for Social Anthropology on the staff of *Sociological Abstracts*.

Southern Methodist University. Walter T. Watson, twice chairman of the Sociology section, has been elected as President of the Southwestern

Social Science Association at its annual meeting in Dallas. Morton B. King, Jr., formerly Visiting Professor at Northwestern, has been appointed Professor in the Department. Bruce M. Pringle has recently been promoted to Associate Professor.

State Teachers College, Dickinson, North Dakota. Erwin F. Karner, formerly of the University of Wisconsin and East Tennessee State College, has been appointed to the faculty.

Temple University. Leonard Blumberg has been elected Chairman of the Department of Sociology and Anthropology, replacing George H. Huganir who is now serving as Vice Provost of the University.

University of Washington. A special program of study for a limited number of selected undergraduates who intend to prepare for professional careers in sociology is now being offered.

Walter B. Watson of the University of Wisconsin has joined the Department as Assistant Professor. Sanford A. Dornbusch, who spent the last three years at Harvard University, will return to the University as Associate Professor. Peter Garabedian, who received his Ph.D. last summer, has been appointed Assistant Professor at the University of Rochester. Ralph Connor has accepted an appointment as Assistant Professor at the University of Illinois. Stanton Wheeler has been appointed Instructor in the Department of Social Relations at Harvard University.

Washington University, St. Louis. Stuart A. Queen, member of the Department for 26 years and its chairman for 24 years, one-time president of the American Sociological Society, author of numerous books and articles, was appointed Professor Emeritus last June. He is now Visiting Professor at the University of Wichita.

David Pittman from Rochester and Albert Wessen from the University of Vermont have joined the Department as assistant professors, and are appointed also to the Social Science Institute and the Department of Psychiatry. Wessen is also on the staff of the Jewish Hospital in St. Louis.

Jules Henry is participating in a study of geriatric nursing as consultant to the University's School of Nursing. The U. S. Public Health Service is supporting the research. During the summer, Henry continued his work with disturbed children at the Sonia Shankman Orthogenic School of the University of Chicago. In October, he will go to Copenhagen as a U. S. delegate to the WHO Conference on Mental Health of Children.

Paul J. Campisi and Robert L. Hamblin are participating in a study of the adjustment of rural migrants in the urban area on a grant by the National Conference of Christians and Jews. Hamblin is also research director of a study in-

vestigating factors of conformity and nonconformity of adolescents. This work is a project of the Social Science Institute with funds from the American Social Hygiene Association.

Joseph A. Kahl spent the summer as a visiting professor at Mexico City College, and continued his research in a Mexican factory on the social effects of industrialization.

Ralph C. Patrick was on leave in 1957-58 to direct production of 36 television programs on values in American life for the Educational Television and Radio Center. The production used KETC, the St. Louis educational station. Patrick is now Associate Professor in the School of Public Health, University of North Carolina.

N. J. Demerath, as consultant for World Health Organization's Pan-American Sanitary Bureau on the administration of social research in international health programs, was in South America for six weeks during the summer. Demerath served as President of the Society for Applied Anthropology in 1957-1958.

Stanley Spector has been appointed Associate Professor of Far Eastern Affairs. In June he began a 15 months' leave of absence on a grant from the Social Science Research Council to study the role of intellectuals in the political life of Singapore and Malaya.

Yale University. Leo Srole of the Department of Psychiatry, Cornell University Medical College, is Visiting Professor during 1958-59.

August B. Hollingshead, last year lectured and conducted research in Great Britain under a Fulbright grant. Maurice R. Davie is on a Fulbright grant to the University of Rome during the current academic year. Leo W. Simmons will be on sabbatical leave during the spring term.

Richard D. Schwartz, on leave during 1957-58 to engage in research in the Yale Law School, has been appointed Assistant Professor of Sociology and Law. Richard J. Coughlin has been granted a year's leave of absence to continue his work with the Asia Foundation in Hong Kong.

Donald Trow has received a grant from the National Institutes of Mental Health for his investigation of Participation and Leadership in Voluntary Associations. Omar Moore's grant from the Office of Naval Research for experimental studies in Problem Solving and Interaction and Charles Snyder's grant from the U. S. Public Health Service for the study of Alcohol and Higher Order Problem Solving have been renewed.

Fellowships under a Commonwealth Fund grant are available for the special two-year training program in Medical Sociology. Fellowships pay 1,500 dollars in the first year and 2,000 dollars the second year, supplemented by tuition scholarships from Yale University. The closing date for applications is February 1. For further information write to A. B. Hollingshead, director of the program, 1965 Yale Station, New Haven, Connecticut.

BOOK REVIEWS

The Myth of the Ruling Class: Gaetano Mosca and the "Elite." With the first English translation of the final version of *The Theory of the Ruling Class*. By JAMES H. MEISEL. Ann Arbor: University of Michigan Press, 1958. vi, 432 pp. \$7.50.

This book is an "unabashed, old-fashioned exercise in cogitation, performed without benefit of any task force or research team." As a consequence, it is an admirable performance in scholarship and in careful moral judgment.

Professor Meisel tells us that he was so impressed by Mosca's major opus (*The Ruling Class*, 1938) that he began to search for other works of Mosca. He found an astonishing amount of worthy writing and he began to think that the best Mosca was the Mosca of the brief essays and articles.

This book, an intellectual biography of the kind so dear to Lord Morley or Lord Cecil, is most readable. Mosca's life is sketched in such a way as to throw the necessary light on his thought. Meisel analyzes his thought and criticizes it by examining all the available Mosca writings in chronological order. What intrigues the author is that Mosca, blessed by a long life, started as a staunch critic of Marx and ended as an equally staunch critic of Mussolini. Yet what makes him not merely interesting and original, but actually intriguing, is that Mosca was always coherent and yet always in evolution.

The author begins at the beginning with the *Teorica dei Governi*, Mosca's first opus, and proceeds through the two editions of the *Eлементи* (published together in the United States as *The Ruling Class*), and then to the *History of Political Theories* and finally to *Parties and Labor Unions in the Crisis of the Parliamentary Regime*, a collection of essays and articles published posthumously (1949).

Meisel observes that Mosca started by criticizing Aristotle, Montesquieu, and Marx, and that after a lifetime of struggle he slowly returned if not to Marx, at least to Aristotle and Montesquieu. Not that he endorsed them with exultant admiration, but what had been criticism became at least toleration. The theory of the ruling class, which Mosca had announced bluntly and almost mechanically, he retouched so much that what was formula at first became a myth, almost a tale at the end of his long life. The man who

had begun as an inflexible positivist ended by being a moralist.

Meisel's criticism is so acute and so understanding that it adds to Mosca rather than detracting from him. Mosca, a major positivist writer, becomes, once Meisel has unfolded his critique, simply a major writer. His relation to other writers of his times, in particular to his rival and arch-enemy Pareto, or to his student and follower Michels, bring both of them to life and they emerge as they were: Pareto as a great, nasty man, and Michels as a little emphatic statistician. And Mosca the complex person, whose thought seems so straight and direct at first glance, reveals many folds at the second, and an infinite number of problems and germs of problems at the third.

Actually Mosca is a moralist, whether he wanted to be or not. He was a moralist not only in his writings but in his own life; he was a moralist in politics, and he was a moralist in his attempt at "substituting a complete positive system for a metaphysical system." His peculiar *forma mentis* was due in large part to the structure of the Italian university system, which holds that all which occurs in society is to be reckoned, meditated, and taught within the limits, narrow to be sure, of a law school. In fact some of the most meaningful of his writings have been patiently identified and located by Meisel in his *nugae*. Preoccupation with his times was for Mosca as strong as his preoccupation with a positivistic science of politics. He learned that, just as an historian like Thucydides who can write the history of his times is rare, so is the political writer who can look at his times with the objective eye of the scientist.

To praise this book is easy indeed: it is well thought out, admirably documented, warmly and soundly written, a book to be considered not merely as a source of learning and intellectual stimulation but of literary enjoyment as well. Meisel's view of Mosca is complete, his view of his times synthetic and accurate, his knowledge of the major political theorists, as seen through Mosca's work, amazingly rich. And his choice of Mosca's most original contributions is equally fine: the doctrine of the ruling class, in all its versions, and the principle of juridical defense—Mosca's superb definition of a government by law. Equally rewarding is his discourse on Mosca the Machiavellian, with the Floren-

tine at his right and James Burnham at his left.

This reviewer is human enough to indulge in pedantry. On page 26 one finds the expression "idée maîtresse." Why this expression with its implications of illegitimacy and piquancy rather than "idée mère," employed so logically by Tocqueville in his notes? On page 175 one reads that *Mind and Society* is Pareto's master work. True, if we are to judge Pareto by his works published in English; I strongly doubt that Pareto the Sociologist is ever on a par with Pareto the Economist. The author of the *Sociologia Generale* is a far inferior writer—as Croce observed—to the author of the *Cours d'Economie Politique* or the *Manuale di Economia Politica*. On page 250 one reads that Trajano Boccalini was a Venetian. Boccalini died in Venice, but he was born in Loreto (a Marchigiano rather than a Venetian) and spent the best part of his years in Rome.

These criticisms are to be taken for what they are: the fruits of this reviewer's pedantry and his concern for perfection in a book which, apart from these *quisquilia*, is to be considered an important contribution to political theory and political sociology.

RENZO SERENO

Johns Hopkins University

Soziale Klassen und Klassenkonflikt in der Industriellen Gesellschaft. By RALF DAHRENDORF. Stuttgart: Ferdinand Enke, 1957. xiii, 270 pp. DM 24.—

This attempt to develop a systematic theory of social classes takes as its point of departure the unfinished last chapter of Marx's *Kapital*. Assembling relevant passages and fragments from a variety of Marx's writings and providing some connective text of his own, Dahrendorf first undertakes to "complete" this chapter. He reconstructs Marx's class theory as an analytical model which he employs in the formulation of a new, presumably more adequate theory. Pointing out that Marx did not intend to describe the concrete structure of industrial society but rather to analyze the causes of its change, the author argues that although Marx's class criteria—private ownership of the means of production—have proven inadequate historically his analytical model ought to be retained: the concept class should be defined as a tool for the analysis of social change, it should not be employed as a descriptive category.

Dahrendorf distinguishes two alternative models for the analysis of social structures: (1) Societies can be viewed as cooperative, integrated social systems, functionally differen-

tiated into a hierarchy of social strata which tend to form a continuum. Social strata consist of aggregates of persons with similar income, prestige, and style of life. The analysis of social strata yields a descriptive picture of the nature of social inequality in a given society but it remains static and does not account for structural change. (2) The second analytical model, on the other hand, is dynamic. It views societies as power structures which consist of conflicting interest groups held together uneasily by constraint yet continually changing under the impact of social conflict. Every power structure always consists of two opposing groups or quasi-groups: those in positions of authority who wield power and those who are excluded from the exercise of authority and are therefore powerless. These two groupings—and only these—the author calls social classes: classes are conflicting groups defined by their position in a power structure.

This analytical distinction between classes as power phenomena and social strata as differential distributions of scarce goods and values is useful, but its value is greatly impaired by the fact that Dahrendorf conceives of the two models as mutually exclusive alternatives. Thus he categorically refuses to analyze the interrelationship between class structures and hierarchies of strata because he considers this irrelevant for theoretical purposes. As a result his treatment of the class structure of contemporary industrial society becomes unnecessarily narrow and formal, bypassing some of the most fascinating substantive problems.

This is painfully evident in the final chapter where the author attempts to apply his theoretical model to an analysis of modern society. Raising the question, "Are there still classes in contemporary society?" he answers that since classes are defined as conflict groups in any power structure, there exist as many class dichotomies in modern society as there are power structures. In industry the superordinate class consists of managers, including all white collar employees, while the manual workers form the subordinate class. The political ruling class he defines in purely formal fashion as the heads of government departments, including the entire administrative bureaucracy, plus the parliamentary deputies of the governing parties. The rest of the citizenry presumably forms a subordinate class. The truly crucial question of the connections between economic and political power structures is dismissed by the arbitrary assertion that these are separate and discrete structures—*quod erat demonstrandum*. His purely formal approach permits the author to conclude that in modern society an individual may simultane-

ously be a member of different classes, depending upon his role in various power structures. In fact, different members of the same family may belong to different classes in the same power structure. To be sure, all this is true by definition but it renders the theory essentially sterile for all substantive purposes. It seems a pity that this book which demonstrates analytical powers of a very high order as well as an impressive command of the literature has been permitted to end on such a note.

KURT B. MAYER

Brown University

Man and People. By JOSE ORTEGA Y GASSET. Translated by WILLARD R. TRASK. New York: W. W. Norton and Co., Inc., 1957. 272 pp. \$4.50.

Since the publisher, on the dust jacket of this book, gives a rather inaccurate picture of Ortega y Gasset, it may be useful to keep in mind that this great Spanish writer was mainly a philosopher. His ideas are to be found in various, occasional, and sometimes unfinished writings—writings which several professors and critics are today putting together to show a coherent and original system of thought.

Ortega undoubtedly deserves a very distinguished place among the major thinkers of our age. Moreover, his concern with social aspects and problems constitutes one of his earliest and more consistent interests. His approach to the social face of human life, not being bound to the professional limitations of the sociologist, shows a notable nimbleness in shifting constantly from one point of view to another: we may consider it a philosophico-social rather than a sociological approach. The very tension between the two terms in the title of this book, "Man" and "People," reflects his main conception of human existence: it transfers to social grounds the cardinal tension (which other contemporary philosophers, such as Heidegger, Jaspers, and Sartre, recognize too, and whose roots are to be found in Christian theology) between deep, true, "authentic" living and superficial or dilapidated living. "We do not actually live our genuine life," writes Ortega. "The genuine reality of human living includes the duty of frequent withdrawal to the solitary depths of oneself." Therefore, each individual fights, each soul is always hesitating between its inner call to collect itself (*ensimismamiento*) and the call from the outside world (*alteración*). The worldly are the others, *people*, that is, the social slope of human life.

Around this anthropo-philosophical axis is organized the analysis of social facts and phe-

nomena that Ortega brings out in order to explain the essence of sociality. One must ask if there exists fair justification for the course reprimand which our author directs to sociologists at large, beginning with Auguste Comte, for having always neglected the definition of society. Ortega was shocked when he found that treatises of sociology failed to say what society is—as books on biology fail to give the notion of life. The philosopher, I suspect, was expecting from science something that science is not supposed to offer: the discussion of the essential nature of its subject, which belongs rather to the philosophical approach. At last Ortega himself is offering such an approach in *Man and People*, although unfortunately the book remained at his death without the polishing touch.

Especially stimulating, in the second half of the work, are the fine "reflections on the salutation," and the chapter devoted to language—where, by the way, the linguistic guild also receives punishment from his hand.

For the student in the social sciences, frequently buried beneath the routines of investigation, of methods, and, sometimes, of petty or senseless problems, the reading of Ortega's book should be very refreshing since it draws attention to the deepest basis of the subject matter he professionally cultivates.

FRANCISCO AYALA

Princeton University

Free Society and Moral Crisis. By ROBERT COOLEY ANGELL. Foreword by REINHOLD NIEBUHR. Ann Arbor: University of Michigan Press, 1958. viii, 252 pp. \$6.00.

This work on the sociology of the moral order grew out of the author's conviction that the greater need at the present time is for clarification of sociological theory rather than for further empirical research. This assessment, following upon the author's completion of his own research on the moral integration of American cities, has led to the present effort "to digest and analyze what social science has to say about the processes and problems of moral order." The result is a book which can provide the general reader as well as the student of sociology with an overview of a great deal of work by sociologists concerned with various facets of the contemporary moral order.

By moral order the author means "the manner in which 'oughtness' is organized." In more analytical terms, the moral order comprises the set of social values about which there is consensus, and the complex of norms and institutions which embody these values and bring them

into actual operation in the society. Problems concerning the proper functioning of the moral order are examined in terms of the kinds of mechanisms which are operative or needed to cope with them. The first set of problems analyzed pertains to socialization of new members of the society into the moral order. The second set of problems concerns the need to control deviance. The author considers mechanisms for reorienting deviant groups, as well as means for retraining individual deviants. A third problem-area is that of maintaining key institutions in good working order, especially their capacity for overcoming recurrent tendencies toward excessive rigidity and the abuse of power. Professionalization of leadership and public monitoring of institutions are the major corrective mechanisms dealt with in this connection. A fourth set of problems concerns adjustment to new conditions, such as the appearance of new inventions or competing values, in a manner which supports the moral order. Collective problem-solving and communication among classes are analyzed in terms of their significance for the readjustment of the moral order.

By using a functional approach to these problems, the author introduces a unity of analysis which raises this work well above the general level of the literature on social problems. However, functional analysis needs to be supplemented by historical analysis, and this would appear to be particularly true in the study of major problems in the operation of the moral order. The author's discussion of the genesis of major threats to the moral order does not possess the unity or clarity attained by his functional analysis of mechanisms which mitigate these threats. But then there are very few efforts to combine systematically functional and historical analysis even in narrow problem-areas, let alone for a problem of such scope and complexity as that of moral order. The author's reliance on the functional approach does have the great advantage of helping to identify levers which people may seize in their defense of core values.

WILLIAM KORNHAUSER
University of California, Berkeley

The Sources of Value. By STEPHEN C. PEPPER. Berkeley and Los Angeles: University of California Press, 1958. xiv, 732 pp. \$8.50.

This is an impressive and important work. Stephen Pepper, a philosopher at the University of California, started it almost two decades ago, and its finished form thoroughly justifies the care and concern that went into its making. Here the point of view expressed in 1947 in

A Digest of Purposive Values is greatly expanded and enriched. While the analysis of purposive behavior is still the central topic, much more attention is now given to issues involving biological, psychological, and sociological material, and the whole study is integrated by a central concept which emerged in the course of the work—that of "selective system." Problem after problem is subjected to clear and illuminating analysis. And while the author is not seeking to give advice, there is much wisdom in the book relevant to the conduct of personal and social affairs. Persons interested in the natural sources of human values, and in the empirical study of such sources, will wish to make this book their own.

The concept of "selective system" is defined as follows: "A selective system is a structural process by which a unitary dynamic agency is channeled in such a way that it generates particular acts, dispositions, or objects (to be called 'trials'), and also activates a specific selective agency (to be called 'the norm') by which some of the trials are rejected and others are incorporated into the dynamic operation of the system" (pp. 667-668). A purposive act is one instance of such a selective system: the purpose of reaching a certain goal both initiates acts that may lead to the goal and selects among these acts in terms of whether they advance or hinder behavior toward the goal. According to Professor Pepper there are a number of other selective systems in addition to the purposive act: the consummatory field, the personal situation, the personality structure, the social situation, the cultural pattern, and natural selection. Every such system contains a natural norm, and corresponding to every such norm there is a species of value. Thus parallel to the seven selective systems listed above—beginning with the purposive act—there are the following seven kinds of value: conative-achievement, affective, prudential, character, social, cultural, and survival. It is the natural norms which the theory of value is to study empirically, and it is these same norms which are "the ultimate empirical justification for evaluative judgments" (p. 287).

It is evident that Pepper's approach to values is in the general tradition of the philosophers R. B. Perry, John Dewey, and C. I. Lewis, and the psychologists E. C. Tolman and Kurt Lewin: values are located in terms of behavior and are open to scientific study; value judgments are empirical and cognitive in nature, and are controllable by reference to "value facts." It seems to me that *The Sources of Value* gives strong support to this general tradition, and marks a substantial advance over earlier formulations of this position.

The isolation of values in terms of selective systems opens up the possibility for further advance in this field by relating value theory to current work in the general theory of systems. W. Ross Ashby, for instance, shows, in *An Introduction to Cybernetics*, how it is possible to analyze the notions which Pepper has been led to in his own studies: "selection," "regulation," "error-controlled regulation," "survival," and the like.

Professor Pepper's concern for an empirical theory of value rightly places him in opposition to the current concentration of many philosophers upon a purely linguistic analysis of such value terms as "good" and "ought." Nevertheless, such analyses, when carried on within the framework of a general theory of signs, seem to me a legitimate and necessary supplement to the work done in *The Sources of Value*.

CHARLES MORRIS

University of Chicago

From Ape to Angel: An Informal History of Social Anthropology. By H. R. HAYS. Drawings by SUE ALLEN. New York: Alfred A. Knopf, 1958. xxii, 440 pp. \$7.50.

This is a fascinating, instructive, and frequently insightful book, and yet in some ways it is a most unfortunate one. Perhaps its virtues should be attributed to the author, who is not an anthropologist but a translator, critic, playwright, and novelist, and its defects blamed on certain difficulties in contemporary anthropology. The book is a popular history of anthropology from about 1820, which attempts the gigantic feat of treating the major figures in the field biographically, with due attention to their intellectual development, their professional careers and their personal lives; it also undertakes to examine dominant theoretical trends and set them in historical context—a sort of combination of professional gossip and sociology of knowledge. It contains truly fascinating personal information about such figures as Bastian, Tylor, Frobenius, and Westermarck, to mention only a few of the biographical vignettes. The biographies alone make the book enjoyable reading and provide interesting and useful information about the conditions which shaped the work of these men. Unfortunately, Hays perpetuates certain longstanding errors and contributes many new errors, some of them unintentionally humorous. His understanding of the relationship between the growth of anthropology and the growth of colonialism is a noteworthy and interesting feature of the work, and there are clear presentations of some of the early controversies

in anthropology. At many points, in the reviewer's opinion, major figures are slighted and minor ones elevated, but rather than discuss particular cases I should prefer to give some attention to the difficulties with Hays' overview of anthropology, man, and culture.

Hays has no understanding of culture as a system, nor of the evolution of culture. This is reflected in his view of Malinowski, whom he admires chiefly because he brought "people" back into the study of culture and because he was interested in applied problems, in his brief and inadequate treatment of Radcliffe-Brown and of British and American structural-functional anthropology in general, and in his chapter on the nature of culture. It is evident in his descriptions of the early evolutionists, in his failure to grasp major contributions of Maine, Tylor, and Morgan, and in his total neglect of modern cultural evolutionary theorists such as Leslie White, Julian Steward, and V. Gordon Childe (although the omission of Childe is not surprising in a work which avoids archeology completely). For Hays, anthropology advances when it deals with contemporary cultures, when it becomes interested in psychoanalysis, and when it pays attention to the individual.

At the same time, Hays is excited by the fact that anthropology is now concerned with applied problems—with efforts to alleviate human misery. He sees it as a potential tool in man's climb from ape to angel, a journey he sees as far from complete. It is somewhat disturbing, however, to find anthropology peddled as a popular panacea in a book which shows understanding neither of the developmental forces which affect culture, nor of the nature of short-term culture change, nor of the nature of cultural systems. One cannot but wonder how an anthropology which is seen as valuable and progressive to the degree that it turns into social psychology can be an instrument for the understanding or controlling of the development of cultural systems or of the relations between such systems. It would seem to the reviewer, however, that the work does reflect reasonably well the views of a fair number of anthropologists regarding the course, present state, and major contributions of the field of anthropology, and hence that Hays is not to be held accountable for the confusion in which the reader is likely to finish the book. *From Ape to Angel* is readable and interesting, too full of inaccuracies of fact and interpretation to recommend to the public or to students, and a sad reminder of anthropology's need for an adequate treatment of its own history.

DAVID F. ABERLE

University of Michigan

Responsibility in Mass Communication. By WILBUR SCHRAMM. New York: Harper and Brothers, 1957. xxiii, 391 pp. \$4.50.

I approached this book warily, being prejudiced against journalism and even more against schools teaching it. I was pleasantly disappointed. Mr. Schramm writes well; he perceives, explores, and vividly illustrates important and generally neglected problems in mass communication. Reinhold Niebuhr's introduction, after a lucid summary, ponders those problems *sub specie aeternitatis*.

Although deplored excesses and deficiencies, Professor Schramm upholds the framework within which mass communications are produced and distributed at present. He hopes that those responsible will become more responsive to (rather vaguely suggested) moral, aesthetic, and social standards. This is to be achieved by exhortation, research, and professionalization.

I share neither Professor Schramm's hope, nor his premises. He came to his conclusions by setting up an unwarranted dilemma: government control *versus* faith in more responsible use of freedom. Yet, I think that it would be quite possible to protect the rights of individuals, which Professor Schramm cherishes no less than I do, without relying either on government control—which we agree would not lead to a net improvement—or on mere faith—which really amounts here to a counsel of despair. For example, I do not see why it should be so hard to protect the individual's right to privacy from the public craving for entertainment, a craving which is exploited by the mass media for sales purposes. Why not prohibit publicizing the sexual activities of any identifiable living person? What public interest would be harmed? (Exceptions might be made when the free consent of all parties involved is obtained, or in connection with some police or court actions.) Equally, the law might bar publication of pictures of identifiable persons without consent or legal warrant. And the libel laws might be strengthened; misquotation and misrepresentation should surely lead to civil liability.

To be sure, with such restrictions, the mass media might become less fascinating. But unlike Professor Schramm, I do not take it for granted that the most widespread use of mass media is always in the public interest. For instance, in Professor Schramm's view, newspapers print news—more or less fairly. I think it is more realistic to regard them as entertainment media. The murders, scandals, divorces, even the pictures of traffic accidents, are selected for entertainment value and not because they are instructive or help inform public opinion. At times they influence; often they amuse; hardly ever

do they inform. In the case of sports, human interest stories, columns, or comic strips, the entertainment purpose is disguised only barely if at all. Even in the case of serious news, the entertainment value greatly influences selection and presentation. Why then should we allow the invasion of something as important, indeed essential, as privacy, in order to jack up the circulation of newspapers? Once we envisage the possibility of restricting the content of the mass media by law—though not, to be sure, through administrative agencies—many, if not all, of the problems Professor Schramm raises will yield. Experience will tell how far. Some problems, such as the vulgarity of entertainment to which Mr. Schramm quite rightly objects, are not, in my opinion, amenable to correction.

Professor Schramm has indeed written an intriguing book. These critical reflections are but a tribute to his work.

ERNEST VAN DEN HAAG
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The New School for Social Research

Politics, Parties, and Pressure Groups. By V. O. KEY, JR. Fourth Edition. New York: Thomas Y. Crowell Co., 1958. xiv, 783 pp. \$6.50.

Shortly after the end of World War II, this reviewer enrolled in a course on American political parties. The text was Professor Key's *Politics, Parties, and Pressure Groups*. It was—at least so it seemed to one callow undergraduate—an excellent book. Now, a dozen years and three editions later, the book is better than ever.

In his Preface, Key asserts that the present edition of *Politics, Parties, and Pressure Groups* is not just a routine revision calculated "to make obsolete the stocks of earlier editions in the hands of second-hand book dealers." He lives up to the promise.

While the basic organization of the book remains largely unchanged, all chapters have been brought up to date, most of them rewritten, and some of them cast in new interpretative frameworks. To mention only a few noteworthy examples: the early chapters on political interest groups are now more firmly anchored in group theory and linked more explicitly to the material on political parties than in earlier editions; his chapter on the nature and functions of parties contains a new analysis of consensus which rescues the concept from the murky limbo in which it usually resides; the treatment of party finance has been drastically altered in light of recent congressional investigations and research, while his discussion of the role of party in the legislative process contains new material on the effects of party affiliation on the voting be-

havior of congressmen. Several chapters draw heavily upon the author's recent investigations into state two-party systems and direct primaries.

This edition also includes three new chapters. One, on congressional nominations, is a pioneering exploration of a largely neglected subject. The two separate chapters on presidential and congressional elections not only skillfully summarize recent voting behavior research but, taken together, provide a refreshing new approach to the old questions of party decentralization and the separation of power.

Methodologically, Professor Key is a middle-of-the-roader. This book does not seek to elaborate a formal theory of political parties, nor is it purely descriptive. It analyses American politics using limited, middle-level theories. This theorizing is both imaginative and characterized by a hard-headed grasp of political realities. His ingenuity in devising indices and tests for his generalizations is impressive. The book's graceful literary style and astringent wit may astonish those unfamiliar with Key's earlier writings.

All sociologists who believe that modern political science is a fuzzy blend of law, history, and philosophy ought to read this book and treat themselves to a pleasant surprise. Those with an interest in political research would do well to study this book with care. Far too much of what passes for political sociology is politically naive and shockingly ignorant of recent advances in political science. Professor Key's book can serve as a potent remedy for both failings.

DONALD R. MATTHEWS

University of North Carolina

Organized Business in France. By HENRY W. EHRMANN. Princeton: Princeton University Press, 1957. xx, 514 pp. \$7.50.

The social problems of post-war France have stimulated much useful research into the actual working of French institutions; such research may one day help to "demythologise" French political doctrines. Professor Ehrmann's book is a careful, well-documented study of the policies and activities of one powerful group in French society. The first part of the book gives a short history of employers' associations from the 1930's, through the period of the Popular Front and under the Vichy régime, up to the liberation of France. The second part is concerned with the structure and activities of present day employers' associations, and includes a

very informative chapter on the connections between business and politics. The third part examines the policies of the associations on a number of specific economic issues, and their attitudes in the matter of industrial relations.

Some important features of the economic and social structure are revealed by this examination. French businessmen have been slow to organize, to establish any relations with the trade unions, and to interest themselves in efficient production, good labor relations or collective bargaining. A 19th century bourgeois mentality has kept them isolated from each other and remote from the despised (and feared) proletariat. Before 1936, the national employers' association comprised chiefly the large corporations and such groups as the *Comité des Forges*. The first real impetus to organize on a wider scale came from the Popular Front government, and business interests then united simply to defend private property against the threat of socialism. Later, businessmen took a prominent part in constructing the Vichy régime's version of the corporate state, and Professor Ehrmann shows that many of the leaders of the post-war employers' movement served their apprenticeship in the Vichy Organization Committees.

All this indicates the deep-rooted class antagonisms in modern French society. Professor Ehrmann indeed sometimes refers to the businessmen as a class, but speaks usually of a "pressure group." The latter term seems inappropriate for a social group which, as this book shows in detail, is chiefly concerned with the defense of a whole economic and social order against other groups which challenge that order. It could properly be applied to the smaller associations in particular industries or regions (such as the alcohol lobby) which pursue narrower and more immediate interests. The significance of class differences is shown again by the difficulties experienced in uniting within a single organization the large corporations and the small and medium producers.

The book is largely descriptive, but it suggests many problems for analysis. In a concluding chapter Professor Ehrmann examines briefly some of the broader implications of his study: the social effects of the co-existence of strong pressure groups and weak governments, the consequences of a tenaciously held ideology hostile to large scale enterprise, and the pervasive influence of class antagonism. Whoever writes the first comprehensive account of the social structure of France will find invaluable material in this book.

T. B. BOTTOMORE

*London School of Economics and
Political Science*

The French Political System. By MAURICE DUVERGER. Translated by BARBARA and ROBERT NORTH. Chicago: University of Chicago Press, 1958. xi, 227 pp. \$4.00.

Monsieur Duverger's book is essentially a text for American students, one of a series designed to provide short yet detailed studies of the politics of various countries.

As a text, the work fulfills many of its functions quite well. Political institutions are described with clarity and brevity as are the histories and programs of the major political parties. There are some errors of fact (for example, in the discussion of the popular front) and some points at which one might differ with the author's emphases (for example, his underplaying of the role of cabinet instability). However, these criticisms do not detract from the essential merit of the institutional portions of the work.

The major criticism which can be levelled against the study is, in fact, that it concentrates almost entirely upon institutional analysis. Aside from the use of some quasi-Marxist vocabulary interlaced with vague references to national character ("the French are individualists," or "the French think in historical terms"), the author makes little attempt to relate political institutions and forces to the social system of which they are part.

And yet Monsieur Duverger is aware that the peculiar form of political institutions is not to be dealt with in this way, that the structure of politics as well as its dynamics can only be understood within a wider social context. For example, he recognizes that the continued existence of a multiplicity of mutually antagonist parties, cabinet instability, and general immobilism are not to be explained in terms of lack of single member plurality electoral districts or the lack of an effective dissolution power. Why, then, this blindness to the more interesting problems which grow out of a study of the relationships between political and social structure?

Part of the answer would seem to lie in the fact that this volume was written as a text, and in this sense the above criticisms are somewhat unfair. On the other hand the full answer probably goes somewhat deeper. Frenchmen share with most other peoples a lack of perspective as regards the basic framework of their social life. Until recently this has been compounded by the fact that France seemed most easily analyzed as that country in which the dilemmas and conflicts in European society were most sharply etched, and where all the logical possibilities of European political thought emerged with sharpest clarity. Thus political

scientists of a non-institutional bent have tended to deal with French politics in terms of the "great issues."

With the erosion of essentially 19th century issues this tendency is on the wane. Scholars are now tending to direct their attention to an examination of some of the really key problems of French political culture. Why the freezing of the pattern of fragmentation so characteristic of France since 1789? Why the continued hypertrophy of ideological nuances? In this effort they have been helped considerably by the systematic study of social systems which derives largely from Weber. Unfortunately, however, to analyze France thus is to lower the status of her political life from tragedy to mere pathos, and this Frenchmen, like the nationals of other countries, find difficult to accept. With the decline of France's real power the tragic and heroic character of her politics represents, more than ever, a self-image of greatness upon which all Frenchmen can agree whatever their differences.

STANLEY ROTHMAN

Smith College

The Japanese Factory: Aspects of Its Social Organization. By JAMES G. ABEGGLEN. Glencoe, Ill.: Free Press, 1958. xiii, 142 pp. \$3.50.

Handbook for Industry Studies. By EVERETT E. HAGEN. Glencoe, Ill.: Free Press, 1958. x, 89 pp. No price indicated.

Mr. Abegglen's volume is a welcome one in the field of Japanese studies and in the growing literature on underdeveloped areas and the process of modernization. It is a sign of progress, not enough for us to be smug about, but progress all the same. The author has spent some considerable time and effort actually looking at the social organization of modern Japanese factories. He presents these observations, uses them to correct what he regards as a previous overestimate of the situation, and draws from this experience some lessons of much wider significance.

Mr. Abegglen has been deeply impressed with how many holdovers from past Japanese social organization remain in great force in modern Japanese factory organization. The most telling observation that he makes is that the formal acquisition of a job, either in the management or the general working force of a Japanese factory, tends to be a lifetime commitment both for the employer and the employee. There is little shifting of firm affiliations, and the relations and obligations on both sides endure even in times of slack demand. Many special aspects of the Japanese employment pic-

ture flow from this. The factory as a social organization often mirrors Japanese family organization. Especially, the factory head is *in loco parentis* as it were. This has in its turn many implications for efficiency judged in strictly technological terms. Nevertheless, the question is raised as to whether Japan could have accomplished what she did had such holdovers not existed. Japan is judged to have been successful specifically because she changed so little. Since she was so successful, the lesson suggested is that other areas in their development may prosper best if the continuities of social structure (even though less efficient technologically) rather than the new ways and attitudes are relied upon.

Mr. Abegglen states on several occasions that the changes even in Japan have been very great, but he is clearly preoccupied by the extent to which this has *not* been so. He feels that the views of other writers, including myself, have overstated the extent of these changes. His emphasis on continuity is on the whole salutary, for much of this literature, even with appropriate caveats, has not devoted sufficient explicit attention to the elements of Japanese social structure which have abided. The emphasis on change seems to be justified insofar as one contrasts what has happened in Japan with the situation in the Tokugawa Period, the basis from which these changes took place. My major criticism of his work on this score would be that: (1) it tends to neglect the importance of the changed position in the general social structure of those patterns with the most easily distinguished pedigrees, and (2) the lesson drawn from this for other areas may be easily overstated. The great value that the study of Japan has for other areas, in my opinion, is not likely to be any easy carry-over of lessons, but merely the perspective it throws on the problem. These other areas are little likely to be able to repeat the Japanese experience. There a very unusual power structure, originally developed from a kinship basis but clearly taking precedence over that base in Tokugawa times, permitted the Japanese to maintain control, but not to hold back the tide—indeed the tides could be accelerated. In this respect the author's emphasis on family analogies, rather than on the place of the family in the general social structure, tends to be misleading.

Professor Hagen's companion volume sets up a system of analysis for industry studies with a view to enhancing their utility for comparative analysis. This system is prefaced by many wise and modest suggestions for students in this field. Categories are neatly arranged, and major terms

are explicitly defined. Students should find it useful, and if many heed its suggestions, later students wishing to generalize from these researches will be the true beneficiaries. As a carper I should much prefer to be taken into the confidence of the author as to the explicit theoretical bases of his system of analysis, but any such careful essay as this is better than none. The next trick is to get it used without becoming a research dictator. In this respect we are particularly fortunate in the character of the author, if one may descend to the *ad hominem* level.

MARION J. LEVY, JR.

Princeton University

Toward the Automatic Factory: A Case Study of Men and Machines. By CHARLES R. WALKER. New Haven: Yale University Press, 1957. xxii, 232 pp. \$5.00.

Automation: A Study of Its Economic and Social Consequences. By FREDERICK POLLACK. Translated by W. O. HENDERSON and W. H. CHALONER. New York: Frederick A. Praeger, Inc., 1957. 276 pp. \$5.00.

These two volumes represent perhaps the polar types of inquiry into the impact of automation in contemporary society. *Toward the Automatic Factory* is a case study of automation in a single mill. *Automation* ventures an over-all survey, based primarily on American data, of the nature and consequences of all those recent technological advances included under the label "automation."

Walker's account of the first four years of operation of a new continuous seamless pipe mill is a welcome addition in a field in which the volume of speculation and opinion, informed and otherwise, far outweighs close and detailed empirical study. Its central problem is the impact of the new technology on a group of workers, in this case on three crews each of which numbered eleven at the outset and was reduced to nine at a crucial point half-way through the study. The workers were formally interviewed during the first year of operation, at the beginning of the third, and toward the end of the fourth. During the second year there were repeated interviews with all levels of management as well as with the work crews.

The analysis concentrates on seven variables: job content, workers' relations with one another and with supervision, working conditions, pay, promotion possibilities and seniority, and the interaction of these variables and their relationship to morale and productivity.

In treating the response to automation as a

long term process rather than as a once-and-for-all reaction, Walker has effectively demonstrated the impossibility of achieving a simple answer to the question of its impact upon industrial workers. The particular sequence of events in this one pipe mill saw "three years of battle between . . . men and a machine, before reconciliation and an equilibrium of forces was achieved" (p. 23). The course and outcome of this sequence was determined by the complex interplay of the demands of the technology, managerial practice and policy, and worker response to the new processes and to management actions. Automated production inevitably created new problems and changed the context in which men worked: it increased the amount of attention and responsibility required of most workers while lessening the sheer physical demands of the job; it limited the possibilities of promotion and affected the pattern of social interaction. But response to these changes varied during the four year period of the study; increased responsibility, for example, elicited complaints at the beginning, but subsequently became a source of considerable personal satisfaction. The key to morale and production seemed to be management policy and actions, although the effects of management practice had to be seen in a context of widespread fear and anxiety concerning the new technology and considerable distrust of management.

In contrast to the more or less narrowly defined problem of *Toward the Automatic Factory*, *Automation* treats in some detail a wide array of problems. Professor Pollock examines the nature of automation in both factory and office, the present and potential rate at which it is likely to be introduced, the problem of manpower displacement and retraining, the impact on managers and workers and on the internal differentiation of the industrial working force, and the effects on big and small business and industrial concentration.

Unfortunately the several chapters seem to have been written at different times and more or less independently of one another. The result is a great deal of backtracking and the loss of a sustained analysis. The quality and character of the analysis also varies widely. Professor Pollock is at his best in dealing with economic issues, even though one might quarrel with some of his conclusions, and at his weakest in examining the social consequences of automation, where he engages in the kind of speculation which only studies of the sort carried on by Mr. Walker can test.

ELY CHINOY

Smith College

Rich Lands and Poor: The Road to World Prosperity. By GUNNAR MYRDAL. World Perspectives, Volume 16. Planned and edited by RUTH NANDA ANSHEN. New York: Harper and Brothers, 1958. 168 pp. \$2.25.

"No critic," Myrdal insists, "has ever been effective without knowing thoroughly what he was criticizing" (p. 106). The authors who have contributed so far to the *World Perspectives* series are without exception men whose long experience and present eminence entitle them to undertake a critical evaluation of their fields and to receive a respectful hearing—Walter Gropius, Paul Tillich, Howard Mumford Jones, V. Gordon Childe, Denis de Rougemont, and others as well known. To this group of "spiritual and intellectual leaders of our epoch who have a paternity in this extension of man's horizons"—as Miss Anshen rhapsodically describes them in a preface which comes close to being a mystique—Gunnar Myrdal has been added to describe and to analyze the wide and increasing economic inequalities which exist between the more and the less developed regions of the world.

This informal little book is actually a revision of the lectures which Myrdal gave late in 1955 in Cairo by invitation of the National Bank of Egypt and which were published by the bank in 1956. The title of the present American edition, *Rich Lands and Poor*, is a far less satisfactory indication of the book's contents than is that of the British edition, which is *Economic Theory and Underdeveloped Regions*. Myrdal's basic concern is with economic theory and with its adequacy as an explanation for economic inequalities and as a framework for constructing effective development policies in the economically disadvantaged countries. Myrdal's basic arguments will be familiar to those who know his viewpoint from his previous writings, especially his books, *An International Economy* and *The Political Element in the Development of Economic Theory*.

In essence Myrdal stresses several related points. Increasingly, economic inequalities and a growing recognition of these are important political realities in the contemporary world. The causes of poverty and lack of development cannot be understood by use of classical economic theories and, in particular, by an assumption that a stable equilibrium exists within and between nations. What is needed is a dynamic approach which sees the economic process as cumulative because of circular causation, which in turn involves an increasing rate of change among all relevant variables whether narrowly economic or not. Such change requires direct "policy interferences" through planning programs to reach national goals; these in turn

demand a sweeping aside of biased and inadequate predilections implicit in an acceptance of such doctrines as *laissez faire* and free trade, in an arbitrary separation of production and distribution spheres, and, in fact, in an economic theory derived from conditions in Europe and other western areas over a relatively short time span. A workable national plan must include effective control of fertility if all its other aspects are not to be jeopardized.

This book exhibits a stimulating, a provocative, and, in many ways, a wise approach to a general theory of economic growth. Despite the centrality of the problem of development to the whole field of economics, no such general theory has yet been derived. What does exist is (1) a series of hypotheses about limited aspects of development or their relations to one another, (2) a group of concepts, such as demand analysis, critical minimum effort, and demographic transition, which are applicable in varying degrees in concrete situations, and (3) a large number of empirical studies of specific areas. Many of the latter have proven useful. Yet they are not scientific in the sense that they have had to be carried on outside a general theoretical framework and have constantly taken as given a host of influential factors which are operating as determinants of the developmental course. Basic questions of values and motivation, for example, have quite generally received only lip service. Yet, as Myrdal properly observes, these and other areas quite outside the range of traditional economic theory are interlocked with economic factors and markedly affect their behavior.

It may be both too simple and too demanding to insist that the "principle of interlocking, circular interdependence within a process of cumulative causation . . . should be the main hypothesis when studying economic underdevelopment and development." But there is no question that this principle is one whose detailed study is of crucial importance. Myrdal again performs the service of questioning traditional approaches and of shaking up established habits of thought. If in one sense much of this has been said before, it is by and large something that deserves being said again and again.

VINCENT HEATH WHITNEY

Brown University

Collectivization of Agriculture in Eastern Europe. Edited by IRWIN T. SANDERS. Lexington: University of Kentucky Press, 1958. x, 214 pp. \$5.00.

In April, 1957, the University of Kentucky sponsored the Conference on Collectivization in

in Eastern Europe, directed by Irwin T. Sanders of the Department of Sociology. Out of it came a number of essays and reports, presented here in seven chapters: "Eastern Europe and World Affairs," by Enno E. Kraehe, "The Peasantries of Eastern Europe," by Irvin T. Sanders, "Collectivization of Agriculture in Soviet Strategy," by Philip E. Mosley, "The Collectivization of Bulgarian Agriculture," by Edmund O. Stillman, "Collectivization in Czechoslovakia and Poland," by Ernest Koenig, "Collectivization in Hungary and Romania," by Nicholas Spulber, and "Collectivization of Agriculture in Yugoslavia," by Jozo Tomasevich. In addition, there are three Appendixes: "Marxist Population Doctrine," by J. Allan Beagle and Ray E. Wakeley, "Mechanization of Agriculture in the Balkans," by Branko M. Pesclj, and "Peasantisms," by Ray E. Wakeley. Basically, the work is a survey of Communist efforts to organize millions of peasants into a standard pattern of production and control; it tries to discover why such regimentation has led to less efficient agriculture from the standpoint of total production although it has facilitated the delivery of produce to state economic enterprises.

May we repeat the standard cliché that some chapters—as in all symposia—are better than others? One or two chapters give the impression that the titles and the authors were included not because they are pertinent to the title of the book but to find a place for them in the scheme of things. But, on the whole, this is a remarkable contribution to the rapidly growing literature on satellite Europe (most of which has suffered qualitatively because of the inclusion of authors whose main qualification consists in being born in one of these captive countries). The editor, in fact, has shown in his previous works and in the handling of his editing assignment here that he heads the procession of real scholars specializing in the comparative study of rural sociology. This is also one of the few outstanding works which sticks to the sociological approach and integrates it with political sociology. The sections dealing with the distinction between ideological claims and "the facts of life" are clear; Wakeley's "Peasantisms" is the clearest summary available of the doctrines of the Central-Eastern-European peasantry. Although there is no systematic bibliography included, many sources of recent information are scattered in most chapters. In short, this is a rewarding volume which handles with sociological competence the analysis of one current stream of Soviet tyranny.

JOSEPH S. ROUCEK

University of Bridgeport

Coloured Minorities in Britain: Studies in British Race Relations based on African, West Indian and Asiatic Immigrants. By SYDNEY COLLINS. London: Lutterworth Press, 1957. 258 pp. 21/- s. net.

As a consequence of immigration the non-white population of Britain has probably trebled in size since the end of the second world war. The increase has been a source of some concern in certain quarters and a number of studies of racial relations in Britain have been undertaken. Nevertheless, it is unlikely that the coloured population exceeds 200,000, or less than a half of one per cent of the total population. In the circumstances it may be felt that British sociologists have devoted a disproportionately large amount of time to studies of racial questions. Existing publications include detailed studies of coloured communities and of particular groups of coloured students and workers. It might be assumed that enough was known already and that further publications in this field would be unlikely to throw fresh light on the subject. However, Dr. Collins' book is a unique and valuable addition to existing studies and fills an important gap in the literature for two reasons. In the first instance it contains the only detailed and systematic study of a Moslem community in Britain. Secondly, it is the first attempt to make comparative studies of the situation in different localities and among different racial, cultural, religious, and national groups. There emerges, therefore, from Dr. Collins' enquiries a much more representative and balanced account of racial relations in Britain than has been available hitherto.

The main part of the book is devoted to an account of research carried out on Tyneside, 1949-1951, and is supplemented with additional data obtained more recently from the same locality and from shorter periods spent in Lancashire and South Wales. The author distinguishes between "Negro," "Moslem," and "Chinese" communities, but each of these may be further subdivided. The Negro group consists of West Indians from a number of places in the Caribbean, together with West Africans from various countries and speaking different languages as their mother tongue. The Moslem group includes Arabs, Somalis, and Pakistanis, while the Chinese community studied contained some immigrants originating from the northern provinces and some from the south, with consequent language differences. There are also first and second generation descendants of all these groups. The outstanding feature, therefore, of the coloured population in Britain is its cultural heterogeneity, making any generalization a virtual impossibility.

Nevertheless, one important difference emerges between the Negroes on the one hand and the Moslem and Chinese on the other. The majority of the former aspired to complete assimilation as individuals and found the petty obstacles put in their way a source of severe frustration. In contrast both the Moslem and the Chinese immigrants exhibited strong internal group solidarity and tended to prefer partial social separation as far as residence and leisure time occupations were concerned.

Dr. Collins draws attention to the part played by what he calls "sponsorship" by white persons in positions of prestige and influence, in facilitating the adjustment and assimilation of coloured immigrants. In the case of the Negro community on Tyneside prominent white citizens were influential and in the case of the Chinese community a particular shipping company made itself responsible for assisting the Chinese immigrants, many of whom were in its employ. It appears that the unfamiliarity of the white majority with the coloured minority combined with a characteristic feeling of reserve towards strangers, makes it doubly necessary that persons having prestige in the white community should actively associate themselves with the coloured minorities in order that whites whose status is less secure are dissuaded from using the minority as a scape goat.

Dr. Collins' study is largely descriptive in character and he eschews any attempt to arrive at theoretical conclusions. He also refrains from presenting case studies of individual immigrants and does not discuss in detail questions of individual adjustment from a social or psychological point of view. An obvious hypothesis emerging from his enquiry would be that, while Negroes endeavouring to assimilate and meeting with resistance are likely to show evidence of deviant behaviour and maladjustment, individual Moslems and Chinese would be better adjusted because they are content not to assimilate. Conflict in the latter case would be more likely to take the form of inter-group clashes. The author does in fact provide evidence of this kind of group conflict in the case of the Moslems, but more information about personality characteristics and adjustment would have been valuable. Probably the fact that Dr. Collins' book is intended for the general reader as well as the social scientist accounts for the absence of theoretical discussion. Despite this omission it is undoubtedly a useful field study, the value of which is enhanced by the fact that the author is himself a West Indian.

ANTHONY H. RICHMOND
University of Edinburgh

The Family Life of Old People: An Inquiry in East London. By PETER TOWNSEND. Glencoe, Ill.: Free Press, 1957. xvi, 284 pp. \$5.00.

This is a study of family life and problems of old people in a long-settled working class borough of Bethnal Green, near Central London. It is the second report of the Institute of Community Studies and a companion study to its earlier publication, *Family and Kinship in East London* by Michael Young and Peter Willmott.

The data for this book were gathered in 1954-55 through interviews with a sample of 203 men and women of pensionable age; two-thirds of the interviewees were women, half of them widowed. Nearly all were manual workers or wives of workers. Their names were obtained from records of general practitioners. Having selected at random seven of the general practices in the borough, the author picked out every tenth card on the doctors' lists and recorded every male patient aged 65 and over and female patients aged 60 and over. Of 261 names thus secured, 58 had to be discarded for one reason or another, leaving a sample of 203 persons. The author claims that about 98 per cent of the population of London is covered by doctors' records and assumes that elderly people of Bethnal Green are similarly covered.

The first ten chapters examine systematically and in quantitative detail the kinship network of the old people: their residence in relation to children and other kin; the ties with sons and with daughters, with brothers and sisters; the family system of care; the nature of reciprocal services which bind generations; man and wife in the home economy; the role of the family as against other social groups in the daily lives of the elderly. The second part of the book treats social problems of old age such as retirement, poverty, isolation, loneliness. The author concludes with some recommendations for social policy.

The sociologist will find the first part of the study especially rewarding. The description of family relations is leisurely and replete with vivid excerpts from interviews. The family system which emerges out of the narrative is different enough from the "middle-class American family" of our textbooks to stir scientific curiosity. To the central problem posed by the author—are old people isolated from their families?—the study gives a negative answer. How close were the family ties of the elderly can be seen from the fact that the interviewees "had an average of 13 relatives within a mile and saw three-fourths of all children, both married and single, once a week, as many as a third of them every day." The family services were not

onesided. Though the old people received much help from their relatives this help was reciprocated, particularly through care of grandchildren who lived in the neighborhood. The author urges that public welfare agencies take cognizance of the strength of these family services in a housing policy which would rehouse people near their relatives.

But apart from this general conclusion as to the continuing importance of generational and kinship ties, the book contains interesting findings on the marked segregation between the roles of husband and wife, the predominance of the "Mum" in the family, the special bond between grandmother, daughter, and daughter's child; the reserve between parent and child-in-law, and others.

This book makes a contribution to the sociology of the family as well as to the study of old age. It is a tribute to it to hope that comparative studies will be undertaken in other regions and other classes.

MIRRA KOMAROVSKY

Barnard College, Columbia University

Free Time: Challenge to Later Maturity. Edited by WILMA DONAHUE, WOODROW W. HUNTER, DOROTHY H. COONS, and HELEN K. MAURICE. Ann Arbor: University of Michigan Press, 1958. xii, 172 pp. \$4.50.

The Aged in American Society. By JOSEPH T. DRAKE. New York: Ronald Press Co., 1958. ix, 431 pp. \$5.50.

In the past decade the stream of publications in gerontology has gradually risen, and although flood tide is probably sometime in the future, the books under review are indications of the state of knowledge and the interest in the problem of aging and the aged in American society. The first book, *Free Time: Challenge to Later Maturity*, is a series of papers originally delivered in 1957 at the University of Michigan's Tenth Annual Conference on Aging. The papers reflect the interdisciplinary nature of the conference, with sociology represented by four contributors. The titles of the papers reflect the focus of the conference: "Aging as a Modern Social Achievement," "American Off Duty," "Preparation for the Leisure of Later Maturity," "The Retired Person and Organizational Activities." The papers are essays of interest to the intelligent reader; they are not intended as detailed reports on research or attempts at conceptual analysis of problems in the field of social gerontology. If the serious reader wished to

pursue any of the subjects further, it would be a rather difficult task, for only two of the twelve authors give references to the literature or sources of citation for facts. Readers of this journal who are nearing middle age or later maturity and would like some intelligent reflections on how to use their free time might find this volume stimulating and useful. One aspect of the book which perplexed this reviewer is why a short series of non-technical papers should require four editors.

Of the four sociologists represented in the volume Friedmann offers the most suggestive paper. He presents a typology of the older worker in which he places the worker in historical perspective in order to understand the meaning of retirement for older persons in the past, the present period, and in future decades.

The volume by Drake is of a completely different character. In the words of the author it can serve "as a college text book for courses of gerontology in geriatrics, as a supplementary text book for courses on population, social problems and social legislation, and as an up-to-date survey for all who are concerned with the problems of the aged." As one might expect in a book of this type, references are carefully given and all pertinent facts receive proper citation.

The book is divided arbitrarily into five sections:

Part I, "Adjustment of the Aged to Societies," is the most sociological section of the book; in it the author compares the aged in agrarian and urban type societies and also summarizes the demographic characteristics of the aging population in the United States.

Part II, "Labor Force Status of the Aged," discusses the aged and the labor force, the difficulty which persons experience in obtaining and holding jobs during later middle age and old age, and the problem of preparation for retirement.

Part III, "Meeting Economic Needs," discusses the assets and income of the aged; over one hundred pages are devoted to the varied programs designed to meet the economic needs of the aged: Old-Age Assistance, Old-Age Survivors Insurance, public retirement systems, and private pension plans.

Part IV, "Characteristics of the Aging," aims to increase our understanding of the "older person as a physical, psychological, and social entity, rather than as a social statistic."

Part V, "Society and Its Aging Population," emphasizes the various types of living arrangements for the aged, and the recreational and educational programs which are designed to

make life more pleasant and meaningful for this age category.

The author says in his Preface that "an effort has been made to place the material in a sociological framework without making it unintelligible to the student not versed in sociology." The author has succeeded very well—some might say too well—for the book is marked by a minimum of sociological insight and analysis. Indeed, with the exception of the use of concepts such as "society," "status," "role," and W. I. Thomas' four wishes, the book does not make many demands upon the reader's knowledge of sociology. The volume is marked by a careful compilation of a variety of factual materials which, in general, are presented in an objective fashion. A rather long bibliography is included which would have been more useful if it had been more systematically organized and annotated.

There is a somewhat naive acceptance of research findings and seemingly little sophistication about the varied interpretations which are possible. For example, on page 321 we are told that church attendance influences a person's level of adjustment, and on the next page the author cites another study which apparently contradicts this statement—yet little attempt is made to interpret or to qualify this discrepancy.

The book has a very strong melioristic tone reminiscent of an earlier period in American sociology. In the section devoted to programs for the aged, the text becomes almost a handbook on how to plan and organize programs for the aged. We learn that clubs for older persons should have one large room and several smaller ones, that "cabins should accommodate from two to four persons and should be warm and well lighted," that "a leisurely program which begins with breakfast at about 7:30 A.M. with a rest period after lunch, and closes with a fireside program of hymn singing and Bible reading at about 9:30 P.M. fits the desires and physical capabilities of most campers." It is the reviewer's opinion, based on interviewing and observing a considerable number of older persons, that many people would not find this type of program in keeping with their interests.

Since this is the first textbook in the field of social gerontology, it is to be expected that it will probably have a wide readership. However, as the field develops it is hoped that other books will be published based on more systematic conceptualizations and analytical research studies.

GORDON F. STREIB
Cornell University

America's Children. By ELEANOR BERNERT. Census Monograph Series. For the Social Science Research Council in cooperation with the U. S. Department of Commerce, Bureau of the Census. New York: John Wiley and Sons, 1958. xiv, 185 pp. \$6.00.

The aim of this monograph is "to distill from a mass of statistical materials some picture of the *interrelatedness* of selected characteristics of children. . . . The scope . . . and the characteristics investigated are necessarily defined and limited by the data utilized" (p. xi). The data are primarily the published materials of the 1950 Census of Population; a few more recent sample studies are included. Thus the study is essentially cross-sectional with the present tense referring to 1950.

The variables initially interrelated are age, sex, region, residence, and color. These then constitute categories for the subsequent analyses of childhood dependency, living and family arrangements, school enrollment and educational attainment, age-grade school progress, and labor force participation. A consistent effort is made to use each completed analysis as a basic variable in examining the next (for example, school progress with reference to labor force participation), and special factors are introduced into some analyses (for example, family income in relation to childhood dependency).

Many of the general findings are well known (for instance, childhood dependency is more characteristic of the South and correlates with low income, poor housing, etc.). But as the analyses of the interrelationships become more complex, much new material is presented. Thus the analysis of school retardation employs new empirical norms developed by Bernert and James N. Ypsilantis. The final chapter on the demographic correlates of labor force participation documents some interesting differences for the various age and sex divisions. Probably the strongest conclusion presented is that, "The continuing streams of migration from farm to city and from areas of comparative disadvantage to areas of superior opportunity make the differences in the training of youth a matter of national interest" (p. 132).

The writing job is difficult in this kind of study, and the writers are to be complimented for their straightforward style. However, the small amounts of new materials presented in the 25 pages of the first four appendices might better have been incorporated into the appropriate chapters. The other technical deficiencies are of lesser consequence.

Bernert closes with the idea that this study raises more questions than it answers and sug-

gests several specific studies to shed more light on some of the questions. In undertaking to present what the census materials do have to say here, however, Dr. Bernert performs an important service. It is doubtful if anyone can read this monograph without becoming impatient to learn what changes a similar study of the 1960 census will indicate.

HARRY V. BALL

Pomona College

Urban Planning and Municipal Public Policy. By DONALD H. WEBSTER. New York: Harper and Brothers, 1958. xii, 572 pp. \$6.00.

Community Organization for Citizen Participation in Urban Renewal. By WILLIAM C. LORING, JR., FRANK L. SWEETSER, and CHARLES F. ERNST. Prepared by Housing Association of Metropolitan Boston for the Massachusetts Department of Commerce. Cambridge: Cambridge Press, Inc., 1957. xxix, 238 pp. No price indicated.

Webster's book is a thorough presentation of all facets of urban planning. Herein planning is related to the structure, functions, and processes of local government together with the legal powers and devices available for putting the plan into effect. Although bridging sociology and political science, this volume will prove of special value to the urban sociologist, for it presents a concise picture of government at the local level within which the various forms of planning must be initiated and developed. The author stresses the role of the planning agency as one of recommendation rather than final action; it is not to supplant the activities of the various municipal departments, but rather to supplement their work and to promote coordination among the plans which they prepare. Although 92 per cent of all cities over 10,000 population carry on some kind of planning there is little uniformity in the organization of the planning agencies, which leads the author to discuss the personnel, and the powers, duties, and procedures of the planning authority at considerable length.

Of importance is the author's discussion of planning the pattern of urban land uses, in which considerable attention is given to street planning for the most effective and economic handling of the city's traffic. Comprehensive planning should extend, however, to diverse municipal services, such as water supply, sewerage systems, the collection and disposal of garbage, ashes and rubbish, fire prevention and protection, safety programs, public health protection, and various cultural programs, including educational and library facilities, museums, and

public recreational programs. In each instance attention is directed to coordination of the individual service, and the methods of financing it.

The book embraces a most practical treatment of zoning as a means of implementing the city plan. Here are discussed the legal status of zoning, the establishment of use zones, and the enforcement of zoning ordinances. Of special value to the urban sociologist is Webster's treatment of urban redevelopment and urban renewal which involve the replanning of those areas in which physical decay has set in. Mention is made of the confusion of terms "redevelopment" and "renewal." The former not only applies to slum clearance and rehousing, but may include complete redevelopment of areas for commerce, industry, expressways, and recreation. Urban renewal is aimed at checking the spread of blight and substituting rehabilitation through measures short of clearance.

Among the conditions for securing federal aid for urban renewal under the Housing Act of 1954 is the development of an official "workable program" which, though left undefined in detail, involves at least some citizen participation. *Community Organization for Citizen Participation* is essentially a commentary on and critique of the use of community organization techniques as these are applied to urban renewal. A series of neighborhoods—stretching from the inner core of the central city to suburban areas eight miles out, and ranging from little or no citizen participation to a voluntary program involving a maximum of citizen initiative—were studied by the authors in order to develop community organizational techniques and to determine their value to the administrator of a renewal program. Using case histories of both local and city-wide attempts at renewal, they depict some of the problems associated with citizen participation.

In Part II the authors generalize on the case histories. The main features of the community with which one must deal in facilitating urban renewal are listed as the economic (housing, business, real estate, etc.), domestic, educational, religious, welfare, and prestige elements. Necessary lines of intercommunication in renewal projects are horizontal, linking citizen groups on one participation plane, and vertical, uniting groups on different levels. Several hypothetical charts show how citizen participation may be organized. Finally, the authors answer the why, when, what, and how of citizen participation from the point of view of the renewal administrator.

EARL E. MUNTZ

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Introductory Sociology. By PAUL H. LANDIS. New York: Ronald Press Co., 1958. xxviii, 726 pp. \$6.50.

An Introduction to the Study of Society. By BLAINE E. MERCER. New York: Harcourt, Brace and Co., 1958. xvi, 640 pp. No price indicated.

This writer started out with the serious intention of reviewing both books. In reading through the volume by Landis, however, the pestering question kept returning: Where have I read this before? In his Preface, Landis states: "The uniqueness of any new textbook for introductory sociology courses must be largely in its organization, emphasis, and student appeal." But this is not a *new* textbook by any measure. The author has taken, page by page, verbatim statements from his *Man in Environment* (Thomas Y. Crowell Co., New York, 1949). In the 1958 book, this reviewer has failed to find a reference to this fact. True, the pages or portions thereof are put together in a new order. There are even occasional new sentences, paragraphs—and probably whole sections of chapters. But essentially, this 1958 book has been reviewed before (by Charles H. Page in the *American Sociological Review*, 15 [February, 1950], pp. 151-152). While not a new book it is certainly a competent one and, in all fairness to the author, its duplications should not prevent instructors in introductory courses from considering it.

Mercer's is a well-written, simple book. It deserves attention particularly among instructors offering one semester courses. Compared with Landis, Mercer is very careful in documenting his statements. Like Landis, and many other authors, Mercer stresses *culture*; fortunately he sees it as a construct and thus avoids the essentialist fallacy. Nonetheless, the fairly lengthy section on basic concepts (Part 2, pp. 24-77) finds little effective use in the remainder of the volume.

Many chapters include "case study" examples, which often consist of quotations from various (but mainly anthropological and journalistic) sources. These examples are useful but their value would have been greatly enhanced had the author himself summarized them with reference to the concepts discussed in Part 2. Because of the manner in which the case studies are presented, they at best tell us that people differ, a point that calls for either much more or much less attention. While in most of his material Mercer strikes a good balance, this is not always the case. For example, one page (227) is devoted

to a quotation regarding the habitat of the Kobuk Eskimo but the sociological relevance of this poetic description is not brought out.

For a book of this size, the coverage is unusual and it is not surprising that the author is confined to the development of concepts, classifications, and descriptions. Relatively little attempt is made to sum up pertinent sociological research

—which might have been possible had the writer restrained some of his obvious interest in cultural anthropology.

The publishers of both books are to be commended for their good taste in the production of these volumes.

JIRI NEHNEVAJSA

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BOOK NOTES

The Idea of Colonialism. Edited by ROBERT STRAUSZ-HUPÉ and HARRY W. HAZARD. New York: Frederick A. Praeger, Inc., 1958. 496 pp. \$5.00.

This collection of fifteen scholarly essays on colonialism was prepared under the direction of the Foreign Policy Research Institute of the University of Pennsylvania. Beginning with Hans Kohn's "Reflections on colonialism," an examination of the myth and reality of this moot term, there follows a section dealing with certain historic colonial patterns, British, French, Russian, Japanese, and American. Part III contributes insight into new trends in opinion and policy. Finally, Part IV offers an assessment of the idea of colonialism. Like all collective undertakings, the contributions are uneven. Of special value for their analysis of the relationships and problems inherent in colonialism are the chapters on "Anticolonialism in Latin America," "Indian attitudes toward colonialism," "The United Nations and colonialism," and the concluding essays, "Colonialism: freedom and responsibility" and "Colonialism reconsidered." The recent political crisis in France makes very timely the essay on "Algeria: a case study in the evolution of a colonial problem." The contradiction presented by a nation steeped in the traditional Leninist interpretation of imperialism but ringed by satellites that stand in the dominant-submissive relationship typical of empire is posed in "Some aspects of Soviet colonialism and anticolonialism." One could wish that Professor Strausz-Hupé, who has in the past contributed substantially to an understanding of geopolitics, had done more than act as co-editor of the volume.—M. A. M.

Algeria: The Realities. By GERMAINE TILLION. New York: Alfred Knopf, 1958. vii, 115 pp. \$2.50.

The author, a French anthropologist specializing in Algerian ethnography, dramatically presents the social and economic "realities" not only of Algeria but of all underdeveloped countries. Her principal themes (which she sometimes treats as her personal discoveries) are the Malthusian dilemma and the integrated nature of social systems. The latter is crucial since it leads her to inveigh against gradual or piecemeal reforms, such as birth control, on the ground that a people can live with reason-

able contentment in an "archaic" peasant society or in an "adapted" industrial society but not in any half-way house. Thus she concludes that "the choice before Algeria is a choice between immediate conversion to an intensive industrial civilization, or a swift decline punctuated by sanguinary interludes" (p. 104). Algerian independence, with its loss of French administration, capital, and citizenship (which enables some 400,000 Algerian Moslems to work in France) would be as disastrous as an attempt to continue colonialism. Her solution is massive economic investment plus thorough-going social reforms sufficient to bring the Moslems up to French standards and so enable them to resist the otherwise shattering effects of European civilization.

This volume can be expected to have a provocative and salubrious effect, not only on the French and Algerians to whom it is addressed, but also, despite its lack of detailed comparative analysis, on American students of acculturation, colonialism, and economic development.—M. S. O.

La Vida Rural Uruguaya. By DANIEL D. VIDART. Montevideo: Departamento de Sociología Rural, 1955. 211 pp. No price indicated, paper.

In this volume Dr. Vidart, Chief of the Department of Rural Sociology of the Uruguayan Ministry of Agriculture, presents a series of essays written in a literary rather than a scientific vein about the variegated aspects of rural life in his country. Nevertheless, much that is of interest is contained between the covers. The material falls into three general categories: the geographical scene, historical processes, and socio-cultural types. In it are to be found social history, folklore, social change, and a gamut of other subjects. Necessarily touching but lightly on each of these topics, a remarkably comprehensive view of rural life in Uruguay is presented in an interesting and very readable style.—J. V. D. SAUNDERS

India's Changing Villages: Human Factors in Community Development. By S. C. DUBE. Ithaca: Cornell University Press, 1958. xii, 230 pp. \$3.50.

The first eighteen months in the life of a Community Development Project in western

Uttar Pradesh are described and discussed in this volume. The plans made, the activities undertaken, the Project's successes and its failures, are all considered by Professor Dube. There are chapters on the villagers' reactions to the Project, on problems which existed in the bureaucracy, and on relations between the two. The book concludes with two short chapters of more general comment. An appendix discusses at length the role of the Village Level Worker, on whose activities a large part of the Development Projects' success depends. Another appendix presents the diary, for ten days, of one Village Level Worker's activities.

In general, Dube maintains a detached and critical view of his material. The book is often overly laconic, and might profitably have been longer. It nevertheless provides much useful material, of interest not merely to Indianists but also to those concerned with governmental planning and the problems of bureaucracy, and the role of social scientists therein.—A. H.

Naven: A Survey of the Problems suggested by a Composite Picture of the Culture of a New Guinea Tribe drawn from Three Points of View. By GREGORY BATESON. Second Edition. Stanford, Cal.: Stanford University Press, 1958. xix, 312 pp. \$6.00.

We, The Tikopia: A Sociological Study of Kinship in Primitive Polynesia. By RAYMOND FIRTH. With a Preface by BRONISLAW MALINOWSKI. Second Edition. London: George Allen and Unwin Ltd. Distributed by Macmillan Co., 1958. xxvi, 605 pp. \$7.50.

A Black Civilization: A Social Study of an Australian Tribe. By W. LLOYD WARNER. Revised Edition. New York: Harper and Brothers, 1958. xx, 618 pp. \$6.50.

Reissue in the same year of these books, originally published at nearly the same time (the first two in 1936, the third in 1937), is a welcome event, for each is generally recognized by anthropologists as a classic monograph, having stimulated students and field-workers alike. All have been out of print for some time.

The original texts remain unaltered, but *We, The Tikopia* has fewer plates than formerly. Each author has made some additions. Bateson has written an "Epilogue 1958," a thoughtful evaluation of his own book (primarily in the light of developments in cybernetic theory), that is of considerable interest. In a new preface, Firth notes where he would, if writing now, include additional material and alter his terminology. He indicates his hope that several works long planned will yet be completed, and lists

the other publications on Tikopia stemming from his original fieldwork, and from the more recent re-study undertaken with J. Spillius. In his new preface, Warner refers briefly to the major issues in the controversy which his treatment of Murngin kinship provoked, reserving fuller treatment of the larger problems involved for a book on the family and kinship structure. A new appendix, described as "A section on some incidents in the life of a Murngin man, Mahkarolla . . ." is a sensitive tribute to him as well.

These books have already proved their usefulness to sociologists concerned with comparative problems. They will no doubt continue to do so in this and in other ways.—A. H.

Sociology. By WILLIAM F. OGBURN and MEYER F. NIMKOFF. Third Edition. Boston: Houghton Mifflin Co., 1958. x, 756 pp. \$6.75.

Many who cut their sociological teeth on Ogburn and Nimkoff in 1940 have offspring likely to go through the same experience soon. These parents, leafing through the current edition, should feel at home. The same usefully flexible organization, a similar approach, and many of the same illustrations will be apparent. A successful formula is not discarded lightly.

A comparison of the second and third editions shows that the book has been checked over thoroughly. Innumerable alterations have been made to bring it up to date, several chapters have been rewritten, and one chapter is new. Terminology has been modified to suit current sociological fashion and the figures and illustrations, increased by one third, are well fitted to the text.

The book remains more notable for the richness of its data than for its theory. It is not Kingsley Davis' *Human Society* nor is it meant to be. As the preface to each edition indicated, the book is aimed at those students who will not go on to professional sociology as well as those who will proceed to further sociological pleasures. In short, it provides what professional writers sometimes call "a good opening hook" to sociology.—BULKELEY SMITH, JR.

Otto Rank: A Biographical Study Based on Notebooks, Letters, Collected Writings, Therapeutic Achievements and Personal Associations. By JESSIE TAFT. New York: Julian Press, Inc., 1958. xix, 299 pp. \$6.50.

Because of her long association with Rank and close identification with his therapeutic approach and theories, Dr. Taft is the logical person to attempt a biographical study of this type. Her account of the early years of his life

is based entirely on "Daybooks" which he kept during his 19th and 20th years, and the extensive quotes from them give fascinating insight into the period of emotional and intellectual turmoil which preceded his association with Freud and the psychoanalytic movement. His association with Freud extended from 1905 to 1926, during which time his abilities so impressed Freud that he lamented the fact that Rank's lack of M.D. precluded his being made heir as leader of the psychoanalytic movement.

This section of the book in particular will be of interest to readers of Jones' biography of Freud for the different light it sheds on the relationships among members of the famous Committee. For the period following Rank's break with Freud Dr. Taft relies primarily on her own contacts and correspondence with Rank. One should not approach this book expecting a critical appraisal of Rank and his work. This is, as would be expected, a labor of love.—DILMAN J. DOLAND

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(Listing of a publication below does not preclude its subsequent review)

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- AUSUBEL, DAVID P.** *Drug Addiction: Physiological, Psychological, and Sociological Aspects.* New York: Random House, 1958. 126 pp. Ninety-five cents, paper.
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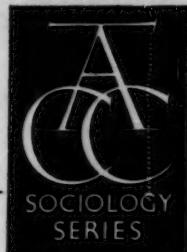
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